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Breaking Barriers: Exploring the Intersectionality of Minority Status, Migration Fear, and Diversity

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BREAKING BARRIERS: EXPLORING THE INTERSECTIONALITY OF MINORITY STATUS, MIGRATION FEAR, AND DIVERSITY

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ABSTRACT

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May 2024

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This dissertation consists of three essays that examine the intersectionality of minority status, migration fear, and racial diversity: 1) The first essay discusses the importance of ethnic diversity in financial advisory firms. Studies illustrate examples of taste-based discrimination and the challenges faced by minorities in accessing capital. Furthermore, studies conclude a lack of trust in financial advisors, despite their significant role in the financial industry. This essay examines how racial diversity in advisory firms can foster representation and guidance for both minority and non-minority clients. 2) The second essay examines the association between minority entrepreneurial success and migration fear.

Studies illustrate a spike in political discourse against immigrants and immigration. The financial and psychological challenges that immigrants are compelled to face are prominent. I demonstrate how such hostility witnessed during the last decade is responsible for detrimental effects on minority entrepreneurs seeking financial capital. I provide evidence that hostility against immigrants has caused funding shortfalls for minority entrepreneurs in a crowdfunding setting. 3) This essay investigates the negative consequences of migration-related fear on minority financial analysts, uncovering that a rise in the fear index is associated with an increase in both the magnitude of forecast errors and the level of forecast pessimism among minority analysts. Split sample analysis highlights the significant roles played by the information environment, analyst experience, and gender.

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CHAPTER 1

INTRODUCTION

This dissertation consists of three essays that examine the intersectionality of minority status, migration fear, and racial diversity.

In recent years, a growing body of scholarly literature and increased media attention have converged to shed light on the significant implications associated with diversity across various contexts. Particularly, racial and gender diversity have emerged as captivating subjects of investigation among a diverse range of social scientists, including economists and psychologists. Extensive research provides substantial evidence regarding the profound impact of diversity on multiple aspects, including the formation of workplace culture (Liu, 2016), the dynamics of social learning among peers (Bertrand, Luttmer, and Mullainathan, 2000; Pool, Stoffman, and Yonker, 2015), the establishment of trust and communication patterns among individuals (Gompers et al., 2016; Algan, Hemet, and Laitin, 2016), and the composition of skill sets and talents within groups (Page, 2007; Stahl, Maznevski, Voigt, and Jonsen, 2009).

Nevertheless, the exploration of diversity within the finance and economics realm has revealed its complex nature, demonstrating a mixture of both positive and negative effects. On one hand, diversity has been found to enrich the social learning environment for individuals by expanding the range of abilities, cognitive approaches, and preferences within a group (Page, 2007; Stahl, Maznevski, Voigt, and Jonsen, 2009; Gompers et al., 2016; Pan, Siegel, Wang, 2017). On the other hand, diversity may also present challenges, such as potential disruptions in decision-making processes, diminished trust, and hindered cooperation among individuals (Algan, Hemet, and Laitin, 2016; Garlappi, Giammarino, and Lazrak, 2017).

Building upon these findings, the intricate and dualistic nature of diversity presents an intriguing puzzle regarding its impact on individual behavior, particularly in relation to fiduciary responsibilities. This research paper seeks to elucidate this question by thoroughly examining the ramifications of workplace diversity on individuals' misconduct behavior. More specifically, I investigate the effects of branch diversity within an advisory firm on the misconduct behavior of financial advisors which unveils unexplored dimensions in the existing literature.

Despite the prevalence of fraudulent behavior and financial misconduct in the financial advisory sector, the market for these services has experienced significant growth, reaching a staggering $57 trillion as of 2023 (Statista Market Insights, 2023). Numerous studies in the literature emphasize the reliance and necessity of financial advisors among retail investors. However, trust in the financial advisory industry has remained elusive over the past decades. Various factors are debated as causes for this lack of trust, including conflicts of interest (e.g., commissions and incentives compromising advisors' objectivity), ineffective communication and trust-building practices, insufficient qualifications and credentials, and mismanagement of client portfolios.

In this essay, I posit that diversity within the workplace of a financial advisor is associated with misconduct behavior. To conduct empirical analysis, I leverage a comprehensive dataset from the Financial Industry Regulatory Authority (FINRA) BrokerCheck website[1] which includes extensive retrospective information pertaining to registered advisory firms, financial advisors, employment histories, as well as details surrounding their misconduct behavior and regulatory actions. The financial advisor data bears noteworthy significance on multiple fronts. First, the economic importance of the advisory industry is well-documented. The employment size of the industry encompasses more than 350,000 advisors[2] and 3,600 advisory firms[3]. Furthermore, prior studies depict the substantial influence of advisors on retail investors and their portfolios (Foerster et al., 2017; Gurun et al., 2021). Second, the abundance of data and its comprehensive structure, surpassing 1.3 million advisors, 15,000 firms, and 65,000 branches, offers a unique opportunity for conducting in-depth analyses at multiple levels: firm, branch, and advisor. Importantly, this unique feature enables the utilization of firm-by-year fixed effects, effectively mitigating endogeneity issues at the firm level and facilitating the creation of matched samples within the same firm and location.

A large, depressing literature documents many persistent challenges that minority business owners in the United States face in access to financial capital (e.g., Blanchflower et al. 2003, Fairlie and Robb 2007). From credit cards to traditional bank loans to venture capital investment, studies consistently measure reduced access and worse terms for minorities (e.g., Chatterji and Seamans 2012, Cook et al. 2022). These funding shortfalls contribute to lower entrepreneurship and undercapitalized businesses for minorities (e.g., Fairlie 1999, Hamilton et al. 2022, Fairlie et al. 2022).

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[1] https://brokercheck.finra.org/
[2] https://www.statista.com/outlook/fmo/wealth-management/financial-advisory/united-states#assets-under-management-aum
[3] https://www.finra.org/media-center/statistics#key

The last decade has also experienced sharp, episodic pushback against migration and minorities, most evident in political movements that led to Brexit and the election of President Donald Trump. Trump launched his campaign by decrying immigration at the border with Mexico and proposing to build a wall to decrease flows, and upon assuming office he subsequently introduced policies like the "Muslim travel ban" and tougher border enforcement. During his campaign and administration, white nationalist movements gained renewed strength and media attention, often using concern over immigration for messaging (Clark, 2020). Recent research links this inflamed political rhetoric to worsening discrimination, such as higher racial profiling in police traffic stops in counties after a Trump political rally (Grosjean et al. 2022) or acts of Asian Hate in Trump-leaning counties after Trump's tweets of the "Chinese Virus" (Cao et al. 2022).

In the second essay, I build upon these studies by exploring how funding opportunities for minorities provided by the crowd-funding site Kickstarter worsened during periods of inflamed political rhetoric and anxiety. In many respects, Kickstarter campaigns have traits that could insulate them from the shocks measured for political events. A majority of financial backers for typical Kickstarter campaigns live more than 50 miles away from the creator they support, tending to reside in big cities like Seattle and New York. Backers are also typically unconnected to the project creators that they support. While Younkin and Kuppuswamy (2018) document unconscious bias against Black creators on Kickstarter, it is unclear whether funding gaps should worsen with elevated national anxiety levels. Distant backers could consciously or unconsciously withdraw support during tense periods from minorities, but they could also choose to step in and show greater support for minority creators during challenging times.

Across 2009-2021, I quantify a strong, negative connection: minorities are less likely to achieve their funding goals on Kickstarter during periods of high anxiety. And the effect is large. My estimates suggest the minority funding gap on Kickstarter more than tripled in its worst quarters compared to periods of low anxiety. In a recent case example, I also show similar shortfalls of funding success for Chinese ethnic creators during the pandemic and episodes of Asian Hate. Importantly, the effects are not limited to minorities located in very conservative counties. I instead show a much more pervasive retraction of financial support draws from conservative and liberal areas of the country.

The relationship between securities returns and the forecasts made by analysts indicates that investors derive crucial insights about forthcoming earnings from these predictions. These forecasts represent a pivotal source of information for decisions related to stock investments. The market's response intensity is significantly influenced by investors' appraisal of an analyst's predictive prowess. However, the efficacy of the insights provided by analysts' forecasts can be attenuated by various external factors beyond their control, particularly when analysts encounter barriers to accessing essential company-specific information necessary for informed forecasting. Extensive research into sell-side analysts highlights the unique obstacles faced by analysts from minority groups. While substantial literature has focused on disparities in employment outcomes, recent investigations have broadened the scope to encompass challenges encountered post-hiring. For instance, Flam et al. (2023) demonstrated that minority financial analysts often have restricted access to managerial information, directly impacting their forecasting abilities. Moreover, Jung et al. (2019) discovered that market reactions are more pronounced to forecast revisions from analysts with surnames perceived as more favorable, suggesting a bias in the reception of their analyses. An ongoing area for inquiry is whether the obstacles and subconscious biases confronting minority sell-side analysts change over time, highlighting a gap in the current understanding that merits further exploration.

The body of psychological research robustly demonstrates a positive correlation between racial inequality and the prevalence of mood disorders, as evidenced by studies from Williams and Collins (1995), Jackson et al. (1996), Landrine and Klonoff (1996), Kessler, Mickelson, and Williams (1999), Chae et al. (2012), and Kang (2020). Studies document the phenomenon where individuals transfer their emotional states to unrelated economic activities. For example, the evaluation of economic prospects can be influenced by personal moods or sentiments, as shown in studies by Wright and Bower (1992), Wann, Dolan, McGeorge, and Allison (1994), Forgas (1995), and Kang (2020). Furthermore, the recent advancements in behavioral finance underscore the profound impact of sentiments, moods, and anxiety on investor behavior, as detailed in the works of Edmans et al. (2022) and Edmans et al. (2007).

Building on these foundational studies, the third essay of this dissertation delves into the impact of public racial sentiment on minority analysts' earnings per share (EPS) forecasts. Specifically, I explore the influence of macro-level racial sentiment, characterized by fear and anxiety, on the quarterly EPS predictions made by sell-side analysts. This investigation seeks to understand how broader societal attitudes towards race may shape financial forecasting within the context of market and firm analysis.

To measure the quarterly macro-level racial sentiment, I utilized the Migration Fear Index developed by Baker, Bloom, and Davis (2015, 2016), which tracks fluctuations in public sentiment regarding immigrants and immigration. This approach is grounded in research that links discussions and perceptions of immigration closely with attitudes towards racial and ethnic groups (Nelson and Kinder, 1996; Citrin, Green, Muste, and Wong, 1997; King, 2000; Kinder, 2003, Law and Zuo, 2021).

Goal of the Dissertation

This dissertation sets out to explore the intricate ways in which diversity, political rhetoric, and societal attitudes intersect to impact the financial sector, specifically through the lenses of financial advisory services, crowdfunding platforms, and financial forecasting by analysts. At its core, the dissertation aims to shed light on how these dynamics collectively influence access to capital, the accuracy of financial predictions, and the ethical landscape of financial practices, particularly for minority groups. Each essay within the dissertation serves a unique purpose: to unravel the complex layers of influence that workplace diversity, national political climates, and public sentiment have on different facets of financial activity. Through rigorous empirical analysis, this work seeks not only to fill existing gaps in literature but also to provide insights into how enhanced diversity and inclusion within the financial sector can lead to more equitable and effective outcomes.

The goal extends beyond analyzing the beneficial impact of diversity within financial advisory firms to include a thorough examination of how broader social and political narratives shape financial opportunities and perceptions. In the crowdfunding context, the dissertation delves into the tangible effects of political rhetoric on the funding success of minority-led projects, highlighting the vulnerability of minority entrepreneurs to shifts in public sentiment. Similarly, the analysis of minority financial analysts focuses on how external pressures and biases can distort financial forecasts, with significant implications for market behavior and investment strategies. By juxtaposing these varied yet interconnected domains, the dissertation underscores the multifaceted role of diversity and public sentiment in shaping the financial landscape. Ultimately, the aim is to catalyze a shift toward more inclusive practices within the financial industry, advocating for policies and approaches that acknowledge and address the nuanced challenges faced by minorities, thereby fostering a more just and resilient financial ecosystem.

Main Findings

In the first essay, to develop a proxy for racial diversity, I adopt a methodology similar to previous studies on diversity (Alesina and La Ferrara, 2005; Algan, Hemet, and Laitin, 2016; Giannetti and Zhao, 2019), wherein I construct a Herfindahl–Hirschman Index to quantify branch diversity based on the ethnic composition of financial advisors within each branch. This measure serves as a proxy for the degree of representativeness of five predetermined ethnicities within each branch of the financial advisory firm. By exploiting branch (firm-by-county), firm-by-year, and county-by-year fixed effects which control for time-variant firm and county level effects and time-invariant branch characteristics, I find that one standard deviation increase in branch diversity (approximately 15%) is associated with 7.93% and 5.17% lower misconduct rate, relative to baseline average, at advisor and branch level respectively. Although the analyses incorporate a broad set of time-varying fixed effects at firm and county level, it is important to acknowledge that endogeneity concerns may still persist regarding branch level characteristics. Therefore, I employ a staggered difference-in-difference approach centered around the death of minority financial advisors, which exogenously decreasing the representativeness of minority groups at a branch. It is worth noting that minority advisors constitute a mere 9.68% of the sample, rendering their replacement exceedingly challenging during such exogenous events.

Moreover, the analyses conducted on split samples reveal noteworthy findings regarding the differential impact of branch diversity. Specifically, the implications of branch diversity are found to be more pronounced among white, male, and experienced advisors. Furthermore, the effect of diversity is amplified in smaller branches, those affiliated with firms exhibiting a higher incidence of misconduct, and branches situated in racially diverse counties. On one hand, the split sample analyses based on ethnicity and county demographics reveal that diversity acts as a mechanism to improve trust and communication for minority clients. On the other hand, other analyses highlight the disciplinary and monitoring role of branch diversity for advisors and branches that inherently exhibit a higher propensity for misconduct. Lastly, further investigations of branch diversity demonstrate that diversified branches benefit from lower rates of branch closures and reduced advisor turnover, which serve as indicators of improved branch performance.

Kickstarter is one of the oldest and largest platforms for crowdfunding (e.g., Mollick 2014). Projects listed on Kickstarter span many forms: for example, funds for a pop-up food truck to open its first physical restaurant; funds to launch a new graphic novel or music album; funds to support a new art project; and funds to support a local dance production. While proposed projects display a wide range of project funding goals, most are conducted over a 30-day period. Supporters of projects receive a specified reward for providing a given level of funding support, and the project only goes forward if the total stated funding goal is achieved.

I quantify whether minority creators had a more difficult time raising sufficient funds to launch their projects during quarters since 2009 characterized by rising hostile rhetoric and political discourse. To measure the discourse, my main estimations use the Migration Fear Index introduced by Baker et al. (2015, 2016) that is built upon text of news articles in the United States. This index more than tripled in value from the start of Trump's campaign until the end of his first year in office.

Focusing on creators residing in the United States, I identify minority creators through their listed names and, in an extension, personal photos. My primary estimations consider Black, Hispanic, and Asian minority groups. I document a significant decrease in the likelihood of minority creators achieving their funding goals when migration fear spikes. A one standard deviation increase in the fear index connects to a 1.9% lower success rate for minority creators to reach their goals, compared to a baseline average of 48.8%. To place this figure in context, the Migration Fear Index in the United States fluctuated by more than four standard deviations during the campaign and early years of the Trump administration. The estimated effect could thus represent as much as a 15% decline in relative terms for a minority project being successful. Measured in a different way, the minority shortfall in reaching funding goals is three times larger in quarters that have a top quartile index value compared to quarters within the same year that have a bottom quartile index value.

The effects are present across minority groups and show an intuitive saliency for the minority group most affected. For my main analysis across 2009-2021, and in particular for the election of President Trump, the funding retractions during period of high values of the Migration Fear Index are sharpest for Hispanic creators. Black creators have lower success rates in general, but their fluctuations over quarters with the Migration Fear Index are weaker. While Asian creators had higher baseline success rates over the 2010s than white creators, they too show declines with rising index values. In an extension, I examine the spike in Asian Hate during the Covid-19 pandemic's first year, linked in backlash following the "Chinese virus" phrasing of President Trump. In this setting, I find that Chinese ethnic creators in the United States are less likely to raise sufficient support for their funding goals, while there is no impact for creators of other ethnicities.

What explains this shortfall? A first hypothesis is that the minority effect is "mechanical" in nature—heightened fear may decrease funding support for certain types of projects, and perhaps minority creators are disproportionately engaged in those efforts. This does not appear to be the case, as the funding shortfalls hold in very detailed econometric specifications that control for many project and creator characteristics. There is also no significant difference in the rate at which minorities are posting projects to Kickstarter. Moreover, robustness checks (e.g., comparisons to indices of policy uncertainty or immigrant fear in other countries, measuring effects across projects of the same creator that were launched at different times, matched sample analyses during 2015-2017) provide confidence that the empirical apparatus isolates an important role of migration fear in the United States.

A second hypothesis is that the funding shortfall represents a localized retraction of support among backers close to minority creators. The personal natures of some Kickstarter campaigns lend themselves to "friends and family" support and/or localized giving, such as raising money for a new bakery or a community dance performance. (Kickstarter projects focus on creative endeavors and businesses, and it does not contain the hardship appeals for financial support common on platforms like GoFundMe.) One hypothesis for the shortfall is that periods with high values of the Migration Fear Index are characterized by greater reluctance of minority or local backers to provide financial support for creator projects, perhaps in parallel with less giving towards other local or affiliated causes, too. Yet, my evidence is inconsistent with this theory. I observe the largest deteriorations in backer support for campaigns with a wide spatial distribution of anticipated backers and in products/states where minority creators have traditionally drawn their most support from white backers. Generally, minority and local backers are too small of a share of Kickstarter project supporters for their retractions to explain the deterioration.

A third hypothesis is that the migration fear yields a broader retraction of support, including from white backers, who constitute most backers on Kickstarter. I find significant evidence for this hypothesis, especially in the relative uniformity of effects across project types and locations. While the effects are somewhat stronger for creators living in deeply conservative counties, I also find sizable impacts in very liberal counties. While backers for minority projects decline in number when migration fear is high, the spatial distribution of those backer is similar to low fear periods. The evidence thus suggests that the withdrawal of support is relatively uniform across potential backers during quarters with elevated levels of the Migration Fear Index, consistent with prior social science studies that document these types of reactions in survey or experimental settings.

It is important to provide three limits to this analysis and its claims. Much of my analysis uses the Baker et al. (2015, 2016) Migration Fear Index built upon media news articles. While I stress test my empirical framework in many ways (e.g., constructing alternative Google Search indices, looking at a pre-post event study around President Trump's election), I do not parse the exact role of the media. The media may only be reflecting the underlying concerns of audiences (perhaps invoked by politicians), may be causing their audience concerns through their reporting, or a combination of these. In an era where much of political discourse and identity politics revolve around national issues and national media outlets, the role of media is tightly wound up with these other factors. My focus instead is to document diligently the high-frequency connections evident in the data.

Second, and related, causal identification is more difficult when tackling national events and their impacts on platforms like Kickstarter compared to local event studies. My analysis takes three steps. I first document the link visually with raw data, which is an important aspect of the study. Next, I test candidate hypotheses using variation within the data, which effectively rule out many potential mechanisms. The hypothesis most consistent with the data is one of national shortfalls in support, and I rule out spatial differences in backer support for minorities across quarters with high and low index values. Showing this spatial similarity is not the same as proving a national response is behind my results, but I believe the weight of the evidence is consistent with a causal story.

Finally, my analysis cannot separate conscious from unconscious racism, and there are likely instances of both in moments with escalating tension. Prior work has shown systemic racial bias in entrepreneurial finance, and I too observe on Kickstarter a baseline funding gap for minority creators even in times of low fear. This could derive from factors like racial bias among potential white backers alongside lower average wealth levels among friends and family for minorities. I advance these studies by showing a more direct, temporal connection of public attitudes to racial funding gaps. As I lack data on potential backers who chose not to fund projects, I do not draw conclusions about the conscious vs. unconscious nature of their responses.

The third essay reveals a significant positive correlation between the inaccuracy and pessimism of forecasts made by minority financial analysts and heightened public levels of fear and anxiety. Over the period from 1990 to 2022, it was observed that minority financial analysts exhibited a notable increase in their absolute forecast error during times of elevated fear and anxiety. Interestingly, this degradation in performance was unidirectional, biasing their forecasts towards greater pessimism. Specifically, an increase of one standard deviation in the Fear Index corresponds to a 1.91% increase in absolute forecast errors and a 18% increase in forecast pessimism compared to the baseline average. These dynamics were not observed among non-minority financial analysts, suggesting that public antagonism negatively impacts the emotional and psychological state of minority analysts, thereby influencing their economic predictions of future firm performance. Further analysis indicates that political discourse that intensifies discrimination has a more significant impact on female analysts, those with less experience, and analysts bearing less favorable surnames. These insights not only highlight the additional pressures faced by certain analysts within the industry, leading to increased anxiety and mood deterioration, but also suggest that the information environment plays a crucial role, as evidenced by split sample analysis.

To substantiate the causal relationship underlying my observations, I adopt a difference-in-differences methodology, drawing inspiration from existing literature, centered around the 2016 presidential election of Donald Trump. A distinctive aspect of his campaign was the rhetoric directed against immigrants, especially those of Hispanic descent. The election served as an exogenous event that precipitated a surge in the U.S. fear index, thereby addressing potential endogeneity concerns within my empirical framework. My analysis reveals that subsequent to Trump's election, the accuracy of earnings per share forecasts by minority financial analysts significantly declined, an effect not mirrored among their non-minority counterparts. Furthermore, I leverage the 2020 instance of heightened discrimination against Asians, triggered by Trump's designation of the COVID virus as the "Chinese virus." This provocative declaration escalated public tensions, culminating in a persistent issue of "Asian Hate." This event provides an opportunity for an empirical examination specifically focused on East Asian minority analysts, serving as an additional means of verification. The ensuing analysis confirms that East Asian analysts encountered a considerable and statistically significant drop in forecast accuracy following the "Asian Hate" phenomenon in 2020, thus reinforcing the causal inference of my findings.

Contribution

This first essay contributes to two strands of literature which respectively centered on diversity and financial advisors by not only providing first large sample evidence on the ample implications of workplace diversity in a novel context but also shedding light on two prominent mechanisms: the augmentation of client representation and the fulfillment of fiduciary responsibilities. The consequential outcomes of this study hold significant implications for practitioners, policymakers, retail investors, and researchers.

In the second essay, I highlight two of the important contributions from my investigations. Within the finance literature, most studies consider racial funding gaps at a macro level or across the terms evident in micro-level instruments. This study sits in between, and I demonstrate how tightly linked minority funding gaps on crowdfunding platforms can be to changes in the surrounding social and political environment at a quarterly frequency. For recent studies of the consequences of inflamed political rhetoric, I also provide novel evidence on how negative effects can occur beyond localized events or within social media platforms. The findings speak to a widespread retraction in support for minorities that emanates equally from potential backers across the country, producing real effects through the finance channel (e.g., King and Levine 1993).

The second essay contributes to this literature by providing an econometric verification of the impact of spiking migration fears and attitudes towards minorities using high-frequency variation. The crowd-funding setting provides novel evidence on these effects in a financial setting that draws support from across the country. I also contribute to this literature by showing material consequences of hostile attitudes for minorities in a business and entrepreneurial setting.[4]

The third essay of this dissertation enriches two bodies of scholarly work: the examination of sell-side financial analysts and the exploration of Diversity, Equity, and Inclusion (DEI) principles. It highlights how exogenous factors can diminish the value of analysts' forecasts by impairing their ability to process relevant company information accurately. Moreover, while existing research extensively documents the obstacles encountered by minorities, including those within the financial analyst community, this paper elucidates the exacerbation of these challenges during periods marked by heightened fear and anxiety. The insights garnered are of critical relevance to a wide array of stakeholders, including policymakers, sell-side analysts, brokerage firms, and investors who rely on analysts' forecasts for making informed decisions. These findings underscore the necessity for strategies that mitigate the impact of external stressors on the analytical process, thereby enhancing the accuracy and reliability of financial forecasts in turbulent times.

[4] While most minorities in my sample are native born, especially for Black creators, this study also contributes to studies of immigrant entrepreneurship (e.g., Hunt 2011; Fairlie and Lofstrom 2015; Wang and Liu 2015; Gompers and Wang 2017; Kerr 2018; Kerr and Kerr 2016, 2020; Brown et al. 2020).

CHAPTER 2

CHALLENGING THE STATUS QUO: RACIAL DIVERSITY AND FINANCIAL ADVISOR MISCONDUCT

Related Literature and Hypothesis Development

Research has consistently demonstrated the two-facet nature of diversity. While diversity can enrich the social learning environment for individuals, it can conversely pose challenges, potentially leading to obstacles in decision-making processes and impairing cooperation and communication among individuals. Giannetti and Zhao (2019) provide evidence that firms with boards composed of individuals from diverse ancestral backgrounds exhibit a greater number of patents with higher citation rates. However, these firms also engage in more board meetings and make decisions that are less predictable. Moreover, Kumar et al. (2022) demonstrates the significant role of social learning in the forecasts of financial analysts, highlighting the higher influence experienced by those who share the same ethnicity as their peers. Similarly, Dimmock et al. (2018) reveal the contagious nature of financial fraud among coworkers, with stronger coworker influence on advisor misconduct observed among those who share the same ethnicity.

Financial misconduct can manifest in different forms of fiduciary duty violations, as identified by the Financial Industry Regulatory Authority (FINRA), which outlines twenty-three distinct categories of disciplinary events related to such violations by financial advisors. These violations encompass regulatory offenses, criminal offenses, and customer disputes. The misconduct behavior of financial advisors can arise from various factors, sharing similarities with other types of misconduct observed in different financial settings, including board of directors' misconduct, corporate scandals, financial misstatements, and scandals in the mortgage industry. Recent studies shed light on factors and mechanisms that contribute to financial advisor misconduct, such as peer influence (Dimmock et al., 2018), personal wealth shocks (Dimmock et al., 2021), misguided investment strategies (Linnainmaa et al., 2021), regulatory oversight (Charoenwong et al., 2017), and the role of trust and client financial sophistication (Gennaioli, Shleifer, and Vishny, 2015; Egan et al., 2019).

Moreover, studies have explored various facets of the financial advisory industry, including employment decisions, client-advisor relationships, and the implications of financial fraud. Foerster et al. (2017) demonstrate the significant influence financial advisors have on their clients' asset allocation decisions. Likewise, Gurun, Stoffman, and Yonker (2021) highlight the strong link between advisors' ability to maintain client relationships and their employment choices, with approximately 40% of client assets following advisors during transitions. Despite the crucial role of financial advisors, misconduct within the profession is prevalent. Hoechle et al. (2015) and Chalmers and Reuter (2020) reveal the detrimental impact of financial advisors on performance when they steer clients towards high-fee products. Additionally, Egan, Matvos, and Seru (2019) uncover that roughly 50% of financial advisors face termination due to misconduct, but the labor market often reinstates these advisors, thus diminishing the disciplinary actions taken by firms. Recently, Egan, Matvos, and Seru (2022) provide evidence that there is misconduct punishment disparity due to in-group favoritism across different gender and ethnic groups among financial advisors.

As a result, the question addressed in this essay holds significant relevance in the context of recent studies investigating the ramifications of diversity. Accordingly, it has the potential to impact the misconduct behavior of financial advisors through various mechanisms. In instances where the findings support a deteriorated social learning environment and diminished trust, it can be conjectured that branch diversity exerts adverse effects on the misconduct behavior of financial advisors, consequently affecting their likelihood to engage in such behavior.

Alternatively, it can be posited that diversity enhances the accumulation of knowledge and skills, thereby fostering improved decision-making among individuals. A recent study conducted by Bernile, Bhagwat, and Yonker (2018) exemplifies that board diversity is associated with reduced volatility and enhanced firm performance. Concurrently, Calder-Wang and Gompers (2021) observe that diversity plays a pivotal role in bolstering decision-making processes and subsequent performance within the venture capital context. Additionally, other studies provide evidence of the catalyzing impact of diversity on corporate innovation (Griffin, Li, and Xu, 2021) as well as economic productivity and development (Ashraf, Galor, Klemp, 2015). Building upon these results, it can be argued that diversity fosters decision-making and information processing in a group environment. This, in turn, has the potential to improve advisor guidance and mitigate potential biases that lead to mismanagement of portfolios. Furthermore, following on from the findings by Dimmock et al. (2018), one could contend that diversity can prevent herding which may bolster fraudulent behavior among financial advisors who share the same ethnic background.

Lastly, it is crucial to consider the dynamics of trust and communication in client-advisor relationships. Extensive research has documented the influence of demographics on social interactions. Shared backgrounds and experiences facilitate ease of communication and understanding among individuals. Consequently, the role of diversity in shaping branch-client relationships becomes paramount. If diversity enhances trust and communication between individuals, one expects similar patterns to emerge in the interactions between advisors and their clients. These implications are particularly significant in the financial sector, which is often portrayed as hostile and challenging in literature. Recent studies by Bartlett et al. (2022) and Gerardi et al. (2023) provide compelling evidence of minorities facing higher premiums in mortgage and auto loan markets, as well as encountering difficulties in accessing capital. Lusardi and Tufano (2009) observe a disparity in financial literacy levels among women and minorities, highlighting the need for greater representation and guidance from advisors for these demographic groups. Furthermore, Ambrose et al. (2021) conclude that taste-based discrimination exists among brokers in mortgage contracts. Consequently, it is plausible to hypothesize that an increased representation of diverse ethnic groups in a firm branch not only enhances advisor guidance but also fosters trust and connection between advisors and clients.

H_1: Diversity helps the fulfillment of fiduciary responsibilities and the augmentation of client representation, thereby mitigating the occurrence of financial misconduct

Data, Sample, and Methodology

The main data utilized in this study were obtained from the Financial Industry Regulatory Authority's (FINRA) BrokerCheck website. The financial advisory industry operates within a stringent regulatory framework, which facilitates access to comprehensive information on registered advisors through FINRA's BrokerCheck website. The BrokerCheck data originates from mandatory filings known as "Form U-4" for financial advisors, which require the disclosure of various information such as advisor name, employment history, employment address, licenses, qualifications, and any mandated disclosures. Similarly, FINRA mandates that advisors disclose information about customer complaints, civil cases, criminal charges, terminations, regulatory actions, and bankruptcies. To collect the necessary data, I used a programming language to scrape the website and gather retrospective information spanning a window from 2000 to 2020. The unique CRD identifier assigned by FINRA for advisors and advisory firms was used to construct the annual panel data. Following Egan, Matvos and Seru (2019), I define my primary measure of misconduct to include customer complaints, civil and criminal legal cases, terminations, and regulatory actions.

According to FINRA's definition, a financial advisory firm may have multiple branch offices, each representing a distinct business address where one or more associated individuals regularly engage in securities transactions or activities aimed at encouraging the purchase or sale of securities[5]. Using unique CRD identifiers and distinct business addresses, I create a branch level panel that gives the opportunity to precisely identify group memberships and co-working environments (Dimmock et al., 2018). Following the literature, I classify advisors who are employed in the same firm and county as co-workers in a branch (Charoenwong et al., 2017; Egan et al., 2019; Dimmock et al., 2021). To ensure accurate measurement of diversity effects and facilitate robust analyses, branches with fewer than three advisors are excluded from the analyses. My final sample includes approximately 1.3 million financial advisors, 15,825 advisory firms, and 66,063 unique branches during the

[5] FINRA Rule 3110 (f)(2)(A). Please see http://finra.complinet.com/en/display/display_main.html?rbid=2403&element_id=11763.

2000-2020 time window.

Variable Construction & Summary Statistics

Although FINRA BrokerCheck data is very comprehensive, information regarding advisor ethnicity is not provided. To overcome this challenge, I use two different name algorithms, NamePrism and Ethnicolr, which are widely used in economics and finance literature to infer advisor ethnicity and gender. NamePrism is a tool for classifying nationality and ethnicity based on name embeddings, developed by Ye et al. (2017) and Ye and Skiena (2019). NamePrism offers APIs for academic and non-commercial purposes, supporting numerous research projects. Notable studies utilizing names to identify or signal minority status include Bertrand and Mullainathan (2004), Fryer and Levitt (2004), and Kline et al. (2021). The NamePrism algorithm employs a naïve Bayes model that utilizes first and last names to infer ethnicity across six categories: White, Black, Hispanic, Asian and Pacific Islander (API), American Indian and Alaska Native (AIAN), and More than Two Race (2PRACE)[6]. The API provides a dictionary of these six ethnicity categories along with their respective probabilities. By extracting the inferred probabilities for advisors, I create a binary indicator variable called "*Minority*". This variable is equal to one if the highest inferred probability corresponds to the Black, Hispanic, or Asian category, and zero otherwise. Similar to NamePrism, Ethnicolr employs deep learning techniques trained on a Census Bureau dataset that captures the racial distribution of last names (Ambekar et al., 2009). In Section 1.4.7 of this study, I conduct a robustness check using a diversity proxy that is measured based on the ethnicities inferred by the Ethnicolr algorithm.

To measure the representation of each inferred ethnicity group from NamePrism within

[6] There are very few 2PRACE cases (approximately 0.01% of sample), which I leave in the baseline category with white advisors when modelling indicator variables for minorities.

each branch for a specific year, I use the Herfindahl–Hirschman Index (HHI) as an indicator of diversity. The HHI is calculated as follows:

$$\textbf{(14)} \qquad Diversity_{ijt} = 1 - [\left(White_{ijt}\right)^2 + \left(Black_{ijt}\right)^2 + \left(Asian_{ijt}\right)^2 + \left(Hispanic_{ijt}\right)^2 + \left(AIAN_{ijt}\right)^2]$$

Diversity$_{ijt}$ represents the level of diversity in branch *I* within firm *j* for year *t*. Each ethnicity in the equation represents the percentage of advisors belonging to the respective racial group within the specific firm-year-branch. An illustrative example of branches, along with their corresponding diversity scores and racial compositions, as well as location and size information, is presented in Table 1. Panel A showcases the ten least diverse branches, while Panel B displays the ten most diverse branches as of 2020. Both panels consider branches with a minimum of 100 advisors in the office. As an example, the Miami branch of New York Life consists of 145 advisors, with approximately 57% identifying as Hispanic, 1% as Asian, and 42% as White. Table 2 presents the summary statistics for the key variables employed in the analysis. Within the sample, advisors are found to be involved in misconduct approximately 0.7% of the time, a proportion that aligns with previous research findings (Egan et al., 2019; Dimmock et al., 2021). On average, a firm comprises 107 advisors, while the typical branch consists of 25 advisors. Moreover, during the observation period, the estimated probability of a branch encountering financial misconduct within a year is approximately 11%.

Results

Advisor level Baseline

To empirically examine the research question, I begin by constructing a baseline specification at the advisor level. The empirical framework is defined as follows:

$$(2) \qquad Misconduct\ Dummy_{qijt} = \alpha + \beta_1 Branch\ Diversity_{ijt} + \theta\ Advisor\ Controls_{qijt} + \lambda\ Branch\ Controls_{ijt} + \omega\ Firm\ Controls_{jt} + \delta\ Fixed\ Effects_{ijt} + \varepsilon_{qijt}$$

Where *q*, *I*, *j*, *t* index advisor, branch, firm, and year respectively. $Misconduct\ Dummy_{qijt}$ is an indicator variable that is equal to 1 if advisor *q* working at branch *I* within firm *j* has a misconduct for year *t*. To address potential confounding factors and account for time-invariant firm and county characteristics, as well as nationwide yearly factors influencing financial misconduct, I include branch (firm-by-county) and year fixed effects. Branch fixed effects capture branch-specific characteristics like location, client base, and local market dynamics, while year fixed effects control for time-varying factors at the national level, such as regulatory changes and macroeconomic trends. These fixed effects help to isolate the relationship between advisor diversity and financial misconduct, while mitigating the influence of other factors.

The regression model also Includes a vector of control variables that address observable advisor, branch, and firm characteristics which may impact misconduct behavior. The advisor level control variables include (*Male)* and (*White)* advisor indicators; (*Prior Misconduct)* indicator that is equal to 1 if an advisor has a record of misconduct in the last three years; (ln(*Experience)*) calculated as the logged measure of the number of years in the FINRA database; (*Series 63)* and (*Series 65&66)* indicators that are equal to 1 when an advisor has the corresponding licenses. In addition, to account for observable firm and branch characteristics that may affect misconduct behavior, I control for historical firm misconduct (*Hist Firm Misc.*) defined as the cumulative percentage of advisors with misconduct records at the firm level over a three-year period; firm size (*ln(Firm Size)*) and branch size (*ln(Branch Size)*) calculated as the logarithm of the number of advisors working in the respective firm and branch; firm age (*ln(Firm Age)*) is controlled for by including the logarithm of the number of years the advisory firm has existed in the FINRA database. These control variables capture relevant firm and branch characteristics that are likely to be associated with misconduct behavior and provide a more comprehensive analysis of the relationship between diversity and financial misconduct.

Table 3 reports the baseline results of estimating Equation (2). In all specifications, I employ a linear probability model in which standard errors are clustered at firm level[7]. In model (1), I include branch and year fixed effects. In model (2) and model (3), I incorporate firm-by-year and county-by-year fixed effects, respectively. The results demonstrate a significant negative association between branch racial diversity and occurrence of misconduct; advisors within more racially diverse branches are less likely to commit misconduct. The results have significant economic implications. The coefficient estimate of branch diversity on advisor misconduct indicates that a one standard deviation increase in racial diversity, which corresponds to approximately a 15% increase, leads to a 7.93% decrease in the likelihood of financial misconduct among advisors, relative to the baseline average. This finding highlights the substantial impact of racial diversity in reducing the occurrence of misconduct within the advisor population. Moreover, the baseline estimates reveal several factors associated with an increased likelihood of financial misconduct among advisors. Specifically, male advisors, those with more experience, prior misconduct records, and holding investment advisor licenses are more prone to engaging in misconduct. These findings align with previous studies by Egan, Matvos, and Seru (2019, 2022).

Additionally, consistent with the research of Honigsberg and Jacob (2021) and Egan,

[7] Results are robust to clustering at advisor level and branch level.

Matvos, and Seru (2022), I observe that minority advisors exhibit a higher propensity for financial misconduct. This finding is intriguing as it suggests that while racial diversity generally reduces the likelihood of delinquency, minority advisors, in particular, demonstrate higher rates of misconduct. There could be two potential mechanisms at play. Firstly, it is possible that minority advisors perceive they will receive less severe punishment due to their underrepresentation in the industry. This can contribute to an increased likelihood of delinquency among minority advisors. Alternatively, the higher misconduct rates among minority advisors may be attributed to factors such as the experience of a more demanding work environment, which necessitates higher productivity from minority advisors, as suggested by Egan, Matvos, and Seru (2022).

Lastly, it is noteworthy that the past occurrences of misconduct at the firm level are significant predictors of advisor misconduct. The positive coefficient associated with historical firm misconduct implies that previous instances of misconduct at the firm level can encourage advisors' propensity to act fraudulently, underscoring the significance of firm culture (Bernheim, 1994).

Branch level Baseline

To provide more comprehensive analysis of the relationship between branch diversity and branch level financial misconduct, similar to equation (2), I construct a branch level panel data and estimate the following empirical specification:

$$(3) \quad Misconduct_{ijt} = \alpha + \beta_1 Branch\ Diversity_{ijt} + \lambda\ Branch\ Controls_{ijt} + \omega\ Firm\ Controls_{jt} + \delta\ Fixed\ Effects_{ijt} + \varepsilon_{ijt}$$

Consistent with the advisor level analyses, I include branch (Firm-by-County) and year fixed effects in all of my specifications to account for unobserved heterogeneity and time-varying factors. The results of these specifications are presented in Table 4. Columns (1) to (3) present the estimates of linear probability regressions for *Misconduct dummy*, while columns (4) to (6) present the estimates using ordinary least squares regressions for *Ln(Misconduct)*. Columns (2), (3), (5), and (6) also incorporate firm-by-year and county-by-year fixed effects to further control for firm-specific and county-specific time-varying factors. Standard errors are clustered at the branch level to address potential correlation within branches.

The results in Table 4 are consistent with those in Table 3, providing further support for a negative association between branch racial diversity and financial misconduct. The coefficient estimates from the linear probability model suggest that a one standard deviation increase in branch diversity, which corresponds to approximately a 14% increase, leads to a 7.28% decrease in the branch misconduct rate compared to the baseline average. Similarly, the ordinary least squares estimates on the logged measure of misconduct indicate that a one standard deviation increase in branch diversity is associated with a 9.27% decrease in the logged number of branch misconduct. These findings highlight the consistent and robust nature of the relationship between branch diversity and financial misconduct as well as the economic importance of the relationship.

Identification

One important consideration that requires attention in this study is the potential endogeneity concern that could affect the relationship between branch diversity and financial misconduct. To address this concern, I adopt a staggered difference-in-difference approach centered around the death of minority financial advisors, which serves as an exogenous shock leading to a decrease in branch diversity due to reduced representation of minorities. Given the limited number of minority financial advisors in the sample (approximately 9.68%), Replacing a minority advisor who exits a firm due to natural causes becomes exceptionally challenging.

As FINRA does not collect information regarding demises of advisors, to identify deceased financial advisors, I firstly construct a subset comprising exiting financial advisors, including their first and last names, work addresses, exit years, and approximate ages[8]. Subsequently, I scraped more than one million online obituaries from the Ancestry.com[9] website. This data source provides comprehensive information about deceased individuals, including their first and last names, residence addresses, age, and year of death. In subsequent steps, I compare the approximate ages of the deceased individuals with the recorded ages of the exiting financial advisors, ensuring that the age of the deceased person is reasonable. Additionally, I use both the work address and residence address as matching criteria to reduce the number of matches to a single one, ensuring that the distance between the two locations is less than 100 miles. It is important to note that while the matching procedure is not flawless and may involve some degree of uncertainty, any erroneous matches remain random and do not introduce bias into the sample. Ultimately, this process enables me to identify approximately 54,000 (3,000) deceased financial (minority) advisors, serving as a vital component of the analysis. The identification strategy is based on the following specification:

$$(4) \qquad Misconduct\ Dummy_{ijt} = \alpha + \beta_1 Treated_{ij}\ X\ After_{ijt} + \lambda\ Branch\ Controls_{ijt} + \omega\ Firm\ Controls_{jt} + \delta\ Fixed\ Effects_{ijt} + \varepsilon_{ijt}$$

[8] To improve identification and minimize irrelevant obituaries, an approximate age is obtained by adding eighteen years to an advisor's years of work. The approximate age requirement ensures that the age of the deceased person is equal to or greater than the calculated approximate age.
[9] For more information about the website, please refer to https://www.ancestry.com/

Where $Treated_{ij}$ is an indicator variable that takes a value of 1 if branch *I* within firm *j* experiences the death of a minority advisor at any time during the sample period. This variable captures the heterogeneity between branches that experience an exogenous shock affecting racial diversity and branches that serve as the control group. In all specifications, I include branch fixed effects which subsumes the *Treated* indicator. The post-indicator variable $After_{ijt}$ takes a value of 1 for branch-year observations occurring after the event year. The variable of interest is the interaction between *Treated* and *After*, enabling the analysis of the exogenous effects of deceased advisors on branch level financial misconduct. Due to the staggered nature of these events, the control sample includes not only branches that did not experience the demise of a minority advisor during the sample period but also branches that would eventually experience the death of a minority advisor but had not yet. Figures 1 and 2 depict the changes in branch diversity between branches that have experienced the loss of a minority advisor due to death and those that have not experienced such an event. The figures conclude that branches are unable to effectively replace the minority advisor within a three-year timeframe following the event.

Table 5 reports the staggered difference-in-difference analysis on the branch level financial misconduct. The first three columns of the table reports linear probability model estimates on misconduct indicator, whereas columns 4 to 6 estimate ordinary least squares regressions for *Log(Misconduct)*. The coefficient of *Treated* indicator is subsumed by branch fixed effects. Columns 2, 3, 5, and 6 also include firm-by-year and county-by-year fixed effects. To establish the causal relationship between diversity and misconduct, it is important to observe an exogenous decrease in diversity leading to an increase in the misconduct rate. The estimated coefficient of the interaction term on both the misconduct indicator and the logged measure of misconduct demonstrates that a decrease in branch racial diversity is associated with an increase in misconduct propensity and the number of misconduct incidents at the branch level[10]. Specifically, the findings indicate a 28.54% increase in the misconduct rate for branches following the death of a minority advisor, relative to the baseline mean, suggesting the causality of the observed relationship.

I conduct additional analysis on the treatment dynamics to investigate whether treated and control branches exhibit similar trends prior to the occurrence of minority advisor demises. Assessing any potential pre-trend is essential as it would violate common trend assumptions underlying my difference-in-difference specification. Table 6 presents the coefficient estimates of treatment dynamics on branch misconduct. Multiple year indicator variables, denoted as Event, are created to represent the respective years relative to the event year. More specifically, *Event [-1]* represents the year one year prior to the event year, while *Event [-2]* represents the year two years prior to the event year. Similarly, *Event [+1]* represents the year one year after the event, and *Event [+2]* represents the year two years after the event. Two additional dummy variables, *Event [-3]* and *Event [+3]*, are included to capture years three or more years before and after the event year. *Event [-3]* is not included in the model due to perfect multicollinearity reasons. Columns (1) to (3) display the estimates of the linear probability model on the misconduct indicator, while columns (4) to (6) present the estimates of the ordinary least squares regressions model on the logged measure of misconduct. All specifications include control variables from equation (4). Columns 2, 3, 5, and 6 also include firm-by-year and county-by-year fixed effects. In each specification, the interacted year indicators preceding the event year are found to be statistically insignificant, indicating that treated and controls branches have common trends prior to the event.

[10] The results are also consistent when examining advisor level panel data.

However, the coefficients become significant and exhibit considerable magnitudes after the event year, particularly in the first and second years following the event. These findings confirm the robustness of the results obtained in the difference-in-differences analysis.

Stacked Difference-in-Difference with Matched Sample

Finally, I define a more refined construction of treatment and control samples. In the original sample, the control group consists of all branches that did not experience the death of a minority advisor or would experience it in the future but had not yet done so. However, a more rigorous analysis requires matching control branches to treated branches based on both observable and unobservable characteristics. Moreover, recent research has shown that employing a stacked difference-in-difference approach can effectively alleviate potential biases that could emerge when using a staggered difference-in-difference methodology (e.g., Cengiz et al., 2019; Deshpande and Li, 2019; and Baker, Larcker, and Wang, 2022). To achieve this, I create subsamples centered around the [-3,+3] event window, where each treated branch is matched with all other eligible control branches that satisfy specific criteria. These criteria include being in the same firm, sharing the same firm and state, and belonging to the same county. This results in three subsamples, each containing pairs of treatment and control branches that meet these conditions. The subsamples are constructed at the branch-year-pair level, where each pair consists of one treatment branch and one control branch. I ensure that each pair exists at least one year before and after the event year. To empirically investigate the relationship, I estimate the following specification:

$$(5) \qquad Misconduct\ Dummy_{ijtp} = \alpha + \beta_1\ Treated_{ijp}\ X\ After_{tp} + \lambda\ Branch\ Controls_{ijtp} + \omega\ Firm\ Controls_{jtp} + \gamma\ Branch\ FE_{ijp} + \theta\ Year\ FE_{tp} + \varepsilon_{ijtp}$$

Where I, j, t, p index for branch, firm, year, and pair respectively. *Treated* and *Post* indicators are subsumed by the fixed effects.

Table 13 presents the estimates of equation (5), showcasing the coefficient estimates of linear probability regressions (columns 1-3) and ordinary least squares regressions (columns 4-6). Column (1) and Column (4) present results for matched pairs within the same firm. Columns (2) and (5) show results for matched pairs within the same county, while Columns (3) and (6) display results for matched pairs within the same firm and state. All specifications include control variables from equation (5) and incorporate branch-by-pair and year-by-pair fixed effects. Standard errors are clustered at branch level.[11] Across all specifications, the estimated coefficient of the interaction term is consistently significant and positive, indicating that an exogenous decrease in branch diversity leads to an increase in branch misconduct rate. This findings provide further evidence on the causality of the relationship between racial diversity and financial misconduct.

Similar to the analysis presented in Table 6, I extend the examination of treatment dynamics to explore whether trend assumptions hold. The coefficient estimates of treatment dynamics on branch misconduct are reported in Table 14. Consistent with Table 6, I create multiple year indicator variables, denoted as Event, to represent the respective years relative to the event year. *Event [-1]* corresponds to the year one year prior to the event year, while *Event [-2]* represents the year two years prior. Similarly, *Event [+1]* denotes the year one year after the event, and *Event [+2]* represents the year two years after. To capture the years that precede and follow the event year by three years, I incorporate two additional dummy variables, *Event [-3]* and *Event [+3]*. Columns (1), (2), and (3) display the estimates of the linear probability model on the misconduct indicator based on treatment-control samples matched on same firm-year, same county-year, and firm-year-state observations,

[11] Results are also consistent when standard errors are clustered at pair level.

respectively. All specifications include the control variables from equation (5), as well as the fixed effects. In each specification, the interacted year indicators preceding the event year are found to be statistically insignificant, indicating that treated and controls branches have common trends prior to the event. Additionally, the coefficients become statistically significant and exhibit substantial magnitudes after the event year, providing evidence in support of the trend assumptions in each specification.

Cross-Sectional Analysis

In this section of the paper, split sample analyses are conducted to explore the potential mechanism underlying the relationship between branch diversity and financial misconduct. The previous analyses have shown results that contradict the first hypothesis, indicating the negative implications of racial diversity on financial misconduct. However, a missing piece of the puzzle remains regarding how branch diversity actually inhibits the occurrence of financial misconduct. To delve deeper into this issue, it becomes crucial to examine whether diversity fosters improved advisor performance within a branch by enhancing some unobservable branch characteristics such as governance and advisor monitoring, or if it is primarily due to enhanced communication and trust between advisors and clients.

By addressing this question, I aim to shed light on the specific pathways through which branch diversity may influence the occurrence of financial misconduct. This analysis will contribute to a better understanding of the underlying mechanisms and provide insights into how diversity can potentially shape the dynamics of advisor performance and client relationships within a branch setting.

Table 7 reports the split sample results both at advisor level (Panel A) and branch level (Panel B) by estimating Equation (2) and Equation (3), respectively. For continuous variables, sample splitting is based on the median level in both panels. All specifications include controls from the respective equations, as well as firm-by-year and county-by-year fixed effects.

Advisor Characteristics

Prior studies indicate that diverse teams tend to make more effective decisions due to the wider range of perspectives and cognitive approaches they bring to the table (Bernile, Bhagwat, and Yonker, 2018; Calder-Wang and Gompers, 2021; Griffin, Li, and Xu, 2021). In the context of financial matters, involving diverse voices in decision-making processes helps identify potential biases, increasing the likelihood of early detection and prevention of misconduct, and ensures more thorough scrutiny of investment strategies and financial advice. Additionally, diversified branches can help mitigate potential herding behavior among advisors concerning financial misconduct (Dimmock et al., 2018). Studies have demonstrated that male and experienced advisors are more prone to engaging in financial misconduct (Egan, Matvos, and Seru, 2019, 2022). Thus, if diversity acts as an additional monitoring and discipline mechanism for financial advisors, the implications of diversity should be more pronounced among male advisors and those with higher levels of experience.

I begin investigating the underlying mechanisms of the relationship between advisor misconduct and branch diversity, focusing on three key classifications: ethnicity, gender, and experience. In panel A of Table 7, columns (3) to (6), I present the split sample results based on gender and advisor experience. The estimated coefficient of branch diversity on financial misconduct is economically and statistically significant for both male advisors and advisors with experience above the median. Specifically, the coefficient is 2.5 times larger and significant for males, and 3 times larger for experienced advisors. The Wald test for coefficient equality provides evidence to reject the null hypothesis that there is no difference in coefficients between the two subgroups at a significance level of 5% and 1%, respectively. These findings suggest that diversity serves as a monitoring mechanism, contributing to the fulfillment of fiduciary responsibilities.

Previous studies have documented the existence of racial discrimination and taste-based discrimination within advisor-client relationships[12]. In such an unfavorable context, the presence of a diverse team of advisors can instill greater confidence and comfort among clients, leading them to entrust their financial matters more readily. Considering the underrepresentation of minority advisors in the industry, diversity plays a crucial role in creating an environment where minority clients feel more at ease disclosing their financial information. These factors contribute to the establishment of a more inclusive and open atmosphere, enabling clients from diverse backgrounds to engage in meaningful discussions regarding their financial needs and goals. Therefore, it is reasonable to anticipate that diversity would contribute to an improved representation of clients, particularly for minority clients.

Moreover, the challenges faced by minorities in access to financial capital is well documented[13]. By having peers from diverse backgrounds, advisors are better equipped to understand and represent the needs of such clients, contributing to improved service provision. Therefore, in addition to gender and advisor experience, I examine the implications of diversity for white and minority advisors. If diversity acts as a mechanism that shapes advisor-client relationships, the effects of diversity on non-minority advisors

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[12] Many studies provide findings on racial discrimination and taste-based discrimination in access to finance. Some examples are Ambrose et al. (2021), Bartlett et al. (2022), Butler et al. (2023)
[13] For example, Blanchflower et al. (2003), Chatterji and Seamans (2012), and Cook, Marx, and Yimfor (2022)

should be more pronounced.

In panel A of Table 7, columns (1) and (2), I present the coefficient estimates of branch diversity on advisor financial misconduct for white and minority advisors, respectively. The coefficient estimate for white advisors is five times larger and statistically significant at a 1% level compared to that for minority advisors. The Wald test for coefficient equality rejects the null hypothesis that the difference in coefficients is zero at a significance level of 5%. These results indicate that diversity also fosters advisor-client relationships, leading to better client representation.

Branch Characteristics

In addition to investigating individual advisors, I explore the mechanisms that shape the relationship between branch level financial misconduct and branch diversity. To further validate and strengthen the findings presented in section 1.4.5.1, I focus on three important factors: branch size, demographic diversity of the branch location, and the firm's misconduct record. Smaller branches may possess certain characteristics that could potentially impact the incidence of misconduct. For instance, smaller branches often cultivate a more intimate and closely-knit environment, which can foster stronger accountability and personal relationships between advisors and clients. This increased accountability can act as a deterrent to misconduct, as advisors have a more direct stake in maintaining trust and reputation within a smaller client base. However, smaller branches may face challenges due to limited resources and less robust compliance infrastructure, making it more difficult to implement effective oversight and monitoring systems. Given that diversity functions as an additional monitoring system, the effects of diversity may be more pronounced in smaller branches. Furthermore, firms with a higher history of misconduct are more likely to experience instances of misconduct (Egan, Matvos, and Seru, 2019). This could be attributed to firm culture or a lack of responsive governance systems. Consequently, split sample analysis should reveal amplified implications for branches within firms that have a higher record of misconduct. Additionally, if diversity indeed enhances client representation, branches located in ethnically diverse counties, where they serve a more diverse client base, should exhibit more pronounced impacts of branch diversity.

Panel B of Table 7 presents the results of the branch level split sample analysis. The panel showcases the coefficient estimates of branch diversity on branch misconduct, with the sample split based on the median anticipated branch size, county diversity, and firm historical misconduct record. Overall, the results indicate that the effects of diversity are both economically and statistically significant, particularly for branches that are small in size, located in counties with greater demographic diversity, and within firms that have a higher record of historical misconduct. The findings from the Wald test, with a significance level of 5%, indicate a rejection of the null hypothesis that the coefficients do not differ among the three subgroups. These findings align with those presented in section 1.4.5.1, suggesting that branch diversity not only serves as an additional monitoring mechanism for advisors but also contributes to the enhancement of client representation.

Further Investigation of Branch Diversity

In this section, I delve into the extended implications of branch diversity on advisory firms by conducting an analysis of how diversity influences branch performance. Given the significant role of diversity in shaping the behavior of advisors and reducing branch misconduct, it is reasonable to expect that branches with higher diversity in their office would experience benefits such as a reduction in customer disputes and an enhancement in their client portfolio. In parallel, when branches fail cultivating diversity within their office, they should experience higher incidence of customer disputes and financial misconduct, thereby increasing the likelihood of regulatory actions taken against these branches. To investigate the impact of diversity on branch performance, I focus on two pivotal mechanisms that serve as proxies for branch prospects: branch closures and advisor turnover. I begin by examining whether branches characterized by lower levels of diversity have a higher probability of branch closures. Furthermore, Previous research by Egan, Matvos, and Seru (2019) has established that engaging in financial misconduct often leads to termination for advisors. Based on this insight, it can be conjectured that if branch diversity acts as a deterrent to customer disputes and fraudulent behavior among financial advisors, branches with higher diversity should experience fewer advisor terminations. In a parallel manner, the positive association between client-advisor relationships and reduced advisor misconduct attributed to branch diversity can foster higher levels of job satisfaction and engagement among advisors, thereby mitigating advisor turnover. By exploring this relationship, I can provided further insights into the potential protective effect of diversity on branch performance.

Table 8 presents the estimates obtained from both linear probability regression and ordinary least squares regression, examining the impact of branch diversity on branch closure, measured using a binary variable, and branch turnover, represented by the percentage of advisors leaving a branch in a given year. The analyses are conducted for time periods *t+1*, *t+2*, and *t+3*.All specifications of the models include control variables from equation (3) as well as branch, year, and firm-by-year fixed effects. Additionally, I account for the characteristics of the branch's surrounding area at the county level, including the natural logarithm of the population, demographic diversity, population change, education level, political affiliation (republican state indicator), and median household income. Standard errors are clustered at the branch level. Consistent across all specifications, the estimated coefficient of branch diversity on both branch closure and turnover is consistently negative and statistically significant at least at the 5% level. These findings indicate that branch diversity positively influences branch performance in subsequent quarters. Specifically, a one standard deviation increase in branch diversity is associated with a 7.33% decrease in the likelihood of branch closures, relative to the baseline average. These results provide evidence that branch diversity contributes to improved branch performance and stability.

Robustness Checks

I conduct a series of robustness tests to ensure the validity of the findings. These tests include using alternative measures of diversity, incorporating advisor fixed effects, conducting firm level analysis, employing alternative name classification algorithms, and addressing potential survivorship bias in the data. In Section 1.4.7.1, I present the estimates of branch diversity constructed based on the Ethnicolr algorithm, as an alternative to the NamePrism algorithm. Additionally, I examine diversity proxied by entropy instead of the Herfindahl-Hirschman Index. Section 1.4.7.2 demonstrates the estimates of diversity on financial misconduct at the advisor level when advisor fixed effects are included in the analysis. Lastly, in Section 1.4.7.3, I address potential survivorship bias and provide robustness checks to ensure the validity of the findings.

Alternative Construction of Racial Diversity

I conduct several additional tests to explore alternative measures of branch level racial diversity. Firstly, I utilize entropy as a measure of branch diversity instead of the Herfindahl-Hirschman Index. Entropy offers a robust mathematical framework for comprehending and quantifying diversity. By assigning a numerical value, entropy captures the level of diversity within groups, reflecting whether categories are evenly distributed or biased. The scale of entropy ranges from 0 to 1, where 0 signifies that all observations belong to the same category, while 1 indicates an even distribution of observations across all categories. The entropy measure is calculated as follows:

$$Entropy_{ijt} = -\sum_{k=1}^{k=5}(\Gamma_{\mathrm{kijt}} * \log_2 \Gamma_{kijt}) \qquad (6)$$

Where *I*, *j*, and *t* index branch, firm, and year accordingly. Γ_{k} denotes the ratio of advisors belonging to one of the five ethnicity groups identified by NamePrism within branch *I*, firm *j* for year *t*. To further enhance the robustness of the analysis, I also construct branch diversity based on an alternative name-classifier called Ethnicolr. Table 9 presents the estimates from a linear probability model examining the relationship between branch misconduct (proxied by a dummy variable) and branch diversity measured by entropy. Similarly, Table 10 displays the estimates of branch diversity (constructed using the Ethnicolr name-classifier) on branch misconduct. All specifications in both tables include branch and year fixed effects. Columns (2) and (3) additionally incorporate firm-by-year and county-by-year fixed effects, respectively. Standard errors are clustered at the branch level. Consistent with previous findings, the coefficients of diversity in all specifications across both tables are consistently negative and statistically significant at the 1% level. These results further support the notion that diversity acts as a hindrance to branch misconduct.

Inclusion of Advisor Fixed Effects

To examine whether the relationship between branch diversity and advisor misconduct is influenced by unobservable time-invariant advisor characteristics, I conduct an additional analysis by incorporating advisor fixed effects into equation (2). The estimates from a linear probability model, presented in Table 11, explore the connection between branch diversity and advisor misconduct using the aforementioned equation. All specifications consider advisor, branch, and year fixed effects, while columns (2) and (3) further include firm-by-year and county-by-year fixed effects, respectively. Standard errors are clustered at the firm level to account for potential correlation. Across all specifications, the estimated coefficient of branch diversity consistently shows a negative and statistically significant relationship, with significance levels of at least 5%. These findings align with the baseline results, indicating that a one standard deviation increase in diversity is associated with a 10.16% decrease in the likelihood of advisor misconduct, compared to the baseline average. Overall, these results provide further evidence that the observed association between branch diversity and advisor misconduct is not driven by time-invariant advisor characteristics.

Addressing Potential Survivorship Bias

One potential concern with the FINRA BrokerCheck dataset is that FINRA does not guarantee full coverage for financial advisors who terminated their registration more than 10 years ago. This issue has been acknowledged and discussed in the existing literature, such as the work of Clifford and Gerken (2021). To address this concern and ensure the robustness of my results, I conducted additional analyses focusing on a sample that covers the last 10 years of the FINRA BrokerCheck dataset. The estimates from a linear probability model examining branch misconduct (measured by a dummy variable) are presented in Table 12. All specifications include the control variables, year and branch fixed effects as outlined in equation (3). Furthermore, columns (2) and (3) include firm-by-year and county-by-year fixed effects, respectively. Standard errors are clustered at the branch level. Across all specifications, the estimated coefficient of branch diversity consistently reveals a negative relationship that is statistically significant at the 1% level. These results provide strong support for the robustness of my findings, indicating that they are not affected by potential survivorship bias.

Conclusion

Although diversity has been extensively discussed in the literature, many unknowns remain regarding its implications for individual behavior, particularly concerning fiduciary responsibilities. The two-fold nature of diversity raises an important question: does diversity foster individuals' commitment to fiduciary responsibilities, or does it exacerbate delinquency? This paper investigates the effects of workplace diversity on individuals' misconduct behavior, specifically within the advisory industry. By examining the impact of racial diversity at the office (branch) level, this study provides the first large-sample evidence in this novel context. The findings reveal that racial diversity within branches impedes financial advisors' propensity to engage in financial misconduct. A one standard deviation increase (approximate 15%) in branch racial diversity decreases the likelihood of financial misconduct by approximately 7.93%, relative to the baseline average. Branch level analysis corroborates these findings, showing a decrease in the probability of financial misconduct by 5.17%. Split-sample analyses further indicate that branch diversity not only acts as a deterrent for financial misconduct but also contributes to an improvement in client representation. While the underlying mechanisms driving this effect require further exploration, one possible explanation is that diversity plays a crucial role in representing minority clients, countering taste-based discrimination prevalent in the advisory industry. Additionally, results demonstrate that diversity enhances branch performance, resulting in fewer closures and advisor turnovers. These findings underscore the significance of inclusive policies in the workplace by highlighting their substantial effects within the advisory industry and shed light on how diversity influences individual behavior regarding fiduciary responsibilities.

Table 1 Example of branch diversity

The table provided below showcases the bottom ten (Panel A) and top ten (Panel B) branches in terms of diversity as of 2020, based on the condition that diversity exists within the branch and the branch size exceeds 100. Each column pertaining to a specific ethnicity denotes the proportion of advisors belonging to the corresponding ethnic group. Diversity represents the HHI based branch diversity. Variable definitions are provided in the Appendix.

Panel A. Least Diverse

Firm	Diversity	State	County	Size	Black	Asian	AIAN	Hispanic	White
State Farm VP MGMT	1.02%	IN	Marion	196	0%	0%	0%	1%	99%
Vining Sparks	1.16%	TN	Shelby	172	0%	1%	0%	0%	99%
Touchstone Securities	1.33%	OH	Hamilton	149	0%	1%	0%	0%	99%
MML Investors Services	1.39%	PA	Delaware	143	0%	0%	0%	1%	99%
LPL Financial	1.44%	PA	Allegheny	138	0%	1%	0%	0%	99%
LPL Financial	1.68%	NC	Mecklenburg	118	0%	1%	0%	0%	99%
Securities America	1.69%	NE	Douglas	117	0%	0%	0%	1%	99%
Buckingham Strategic Wealth	1.69%	MO	Saint Louis	117	0%	1%	0%	0%	99%
Harbour Investments	1.87%	WI	Dane	106	0%	0%	0%	1%	99%
Northwestern Mutual INV	1.87%	MO	Saint Louis	106	0%	0%	0%	1%	99%

Panel B. Most Diverse

Firm	Diversity	State	County	Size	Black	Asian	AIAN	Hispanic	White
Wescom Financial Services	46.59%	CA	Santa Clara	108	2%	14%	0%	14%	70%
BBV Securities	46.62%	CA	San Diego	135	1%	4%	0%	26%	68%
NYlife Securities	46.62%	CA	Orange	126	0%	17%	0%	13%	70%
Transamerica Financial ADVS	49.73%	TX	Harris	147	1%	16%	0%	16%	67%
NYlife Securities	49.73%	FL	Miami-Dade	145	0%	1%	0%	57%	43%
Precept Advisory Group	49.76%	CA	Santa Clara	122	0%	28%	0%	7%	65%
BBV Securities	49.72%	CA	San Diego	143	1%	5%	0%	29%	64%
J.P. Morgan Securities	55.61%	NV	Clark	194	1%	10%	0%	33%	56%
Citigroup Global Markets	55.61%	NJ	Bergen	137	0%	27%	0%	16%	57%
J.P. Morgan Securities	55.61%	NY	New York	170	0%	25%	0%	18%	57%

Table 2 Summary statistics

This table provides the summary statistics of key variables on the 2000-2020 regression sample.

	Level	N	Mean	Std. Dev.	1st Perc.	Median	99th Perc.
Misconduct Dummy	Advisor	13,255,209	.007	.086	0	0	0
Prior Misconduct	Advisor	13,255,209	.016	.124	0	0	1
Experience	Advisor	13,255,209	7.921	5.573	1	7	21
Series 63	Advisor	13,255,209	.721	.448	0	1	1
Series 65-66	Advisor	13,255,209	.451	.498	0	0	1
White	Advisor	13,255,209	.903	.296	0	1	1
Male	Advisor	13,255,209	.743	.437	0	1	1
Misconduct Dummy	Branch	465,891	.109	.311	0	0	1
# of Misconduct	Branch	465,891	.191	1.26	0	0	3
Branch Diversity	Branch	465,891	.077	.139	0	0	.537
Branch Size	Branch	465,891	25.384	143.808	3	7	290
Firm Hist Misconduct	Firm	132,937	.008	.024	0	0	.105
Firm Size	Firm	132,937	106.921	927.487	3	7	1864
Firm Age	Firm	132,937	8.661	5.43	1	8	21

Table 3 Advisor-level baseline

This table reports coefficient estimates of linear probability regressions. *Misconduct dummy* is an indicator variable equal to one if the advisor commits a misconduct. *Branch Diversity* represents the branch diversity measured using the Herfindahl-Hirschman Index. Variable definitions for controls are provided in the appendix. Column (1) includes branch (firm-by-county) and year fixed effects. Columns (2)-(3) further incorporate firm-by-year and county-by-year fixed effects, respectively. Observations are advisor-by-year. Standard errors are clustered at firm level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

	Misconduct Dummy		
VARIABLES	(1)	(2)	(3)
Branch Diversity	-0.004**	-0.003***	-0.004***
	(-2.25)	(-3.41)	(-4.63)
Prior Misc.	0.048***	0.048***	0.048***
	(31.38)	(31.14)	(30.95)
ln(Experience)	0.002***	0.002***	0.002***
	(11.55)	(11.57)	(11.78)
Series 63	0.001**	0.000*	0.000
	(2.17)	(1.67)	(1.64)
Series 65&66	0.002***	0.002***	0.002***
	(6.61)	(7.23)	(7.03)
White	-0.000**	-0.000**	-0.000***
	(-2.34)	(-2.49)	(-2.59)
Male	0.004***	0.004***	0.004***
	(11.96)	(11.78)	(11.90)
Firm Hist Misconduct	0.102***		
	(11.02)		
ln(Branch Size)	0.001*	0.001	0.001***
	(1.88)	(1.41)	(3.07)
ln(Firm Size)	0.001		
	(1.24)		
ln(Firm Age)	-0.001		
	(-0.87)		
Constant	-0.007*	-0.002	-0.003**
	(-1.76)	(-0.93)	(-2.06)
Observations	13,255,209	13,255,209	13,255,209
R-squared	0.023	0.032	0.036
Branch FE	Y	Y	Y
Year FE	Y	Y	Y
Firm-Year FE	N	Y	Y
County-Year FE	N	N	Y

Table 4 Branch-level baseline

This table reports coefficient estimates of linear probability regressions and ordinary least squares regressions. *Misconduct dummy* is an indicator variable equal to one if the branch experiences a misconduct. *Ln(Misconduct)* represents the natural logarithm of the total number of misconducts within a branch. *Branch Diversity* represents the branch diversity measured using the Herfindahl-Hirschman Index. Variable definitions for controls are provided in the appendix. Column (1) and Column (4) include fixed effects for branch (firm-by-county) and year. Columns (2) and (5) further incorporate firm-by-year fixed effects, while Columns (3) and (6) include county-by-year fixed effects, respectively. Observations are branch-by-year. Standard errors are clustered at branch level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

	Misconduct Dummy			Ln(Misconduct)		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Branch Diversity	-0.049***	-0.040***	-0.040***	-0.055***	-0.046***	-0.045***
	(-8.99)	(-6.20)	(-5.86)	(-9.00)	(-7.48)	(-7.02)
Firm Hist Misconduct	0.794***			0.802***		
	(21.62)			(21.75)		
ln(Branch Size)	0.091***	0.091***	0.092***	0.101***	0.100***	0.102***
	(54.41)	(51.58)	(48.51)	(39.33)	(34.80)	(32.61)
ln(Firm Size)	0.011***			0.019***		
	(5.91)			(7.23)		
ln(Firm Age)	-0.010***			-0.020***		
	(-3.96)			(-7.84)		
Constant	-0.154***	-0.079***	-0.080***	-0.218***	-0.113***	-0.118***
	(-13.75)	(-20.49)	(-19.15)	(-14.82)	(-18.14)	(-17.12)
Observations	465,891	465,891	465,891	465,891	465,891	465,891
R-squared	0.327	0.366	0.399	0.451	0.496	0.521
Branch FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Firm-Year FE	N	Y	Y	N	Y	Y
County-Year FE	N	N	Y	N	N	Y

Figure 1. Diversity trends within the same location around event windows

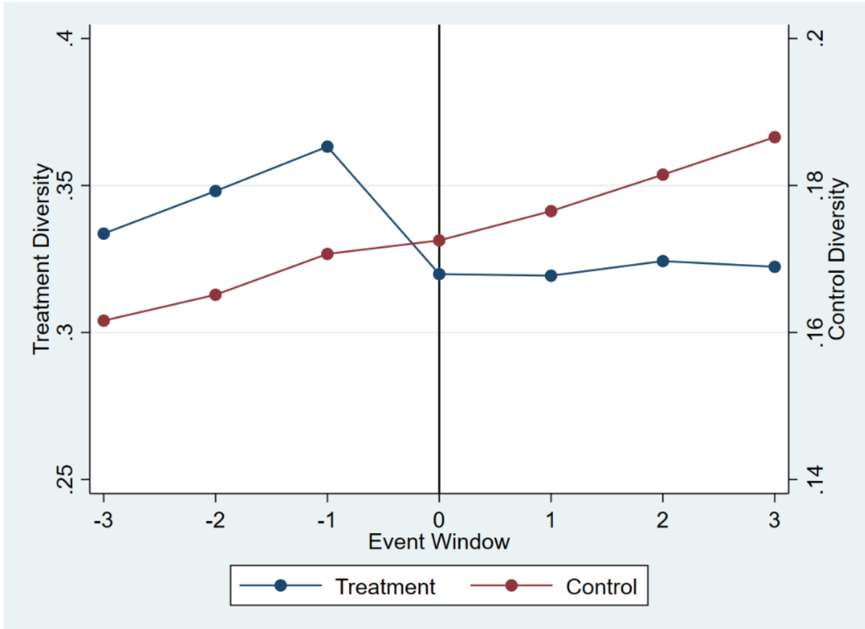

Figure 2. Diversity trends within firms around event windows

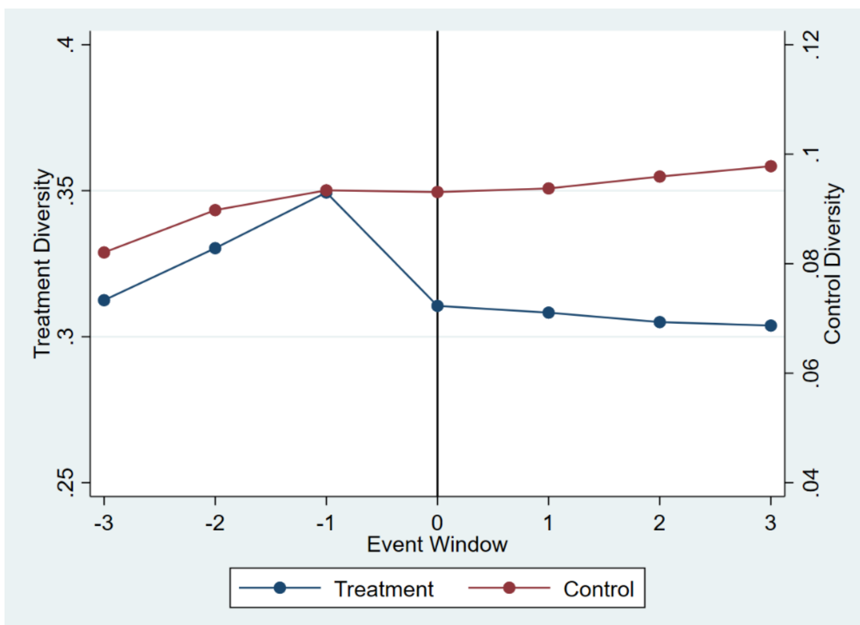

Table 5 Staggered difference-in-difference analysis

This table reports coefficient estimates of linear probability regressions and ordinary least squares regressions. I regress two proxies of financial misconduct on the interaction of *Treated* and *After*. *Treated* is an indicator variable that takes a value of 1 if a branch experiences the death of a minority advisor at any time during the sample period. *After* takes a value of 1 for branch-year observations occurring after the event. *Misconduct dummy* is an indicator variable equal to one if the branch experiences a misconduct. *Ln(Misconduct)* represents the natural logarithm of the total number of misconducts within a branch. Variable definitions for controls are provided in the appendix. Column (1) and Column (4) include fixed effects for branch (firm-by-county) and year. Columns (2) and (5) further incorporate firm-by-year fixed effects, while Columns (3) and (6) include county-by-year fixed effects, respectively. Observations are branch-by-year. Standard errors are clustered at branch level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

	Misconduct Dummy			Ln(Misconduct)		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Treated X After	0.041***	0.038***	0.030***	0.053***	0.058***	0.053***
	(4.68)	(4.18)	(4.27)	(4.04)	(4.52)	(3.88)
Firm Hist Misconduct	0.783***			0.789***		
	(21.32)			(21.37)		
ln(Branch Size)	0.089***	0.090***	0.091***	0.099***	0.098***	0.101***
	(53.74)	(51.15)	(48.26)	(38.98)	(34.60)	(32.51)
ln(Firm Size)	0.011***			0.019***		
	(5.95)			(7.29)		
ln(Firm Age)	-0.010***			-0.020***		
	(-3.94)			(-7.80)		
Constant	-0.158***	-0.083***	-0.085***	-0.223***	-0.117***	-0.125***
	(-14.11)	(-21.26)	(-20.04)	(-15.13)	(-18.60)	(-17.64)
Observations	465,891	465,891	465,891	465,891	465,891	465,891
R-squared	0.327	0.366	0.399	0.452	0.496	0.521
Branch FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Firm-Year FE	N	Y	Y	N	Y	Y
County-Year FE	N	N	Y	N	N	Y

Table 6 Treatment dynamics

This table reports coefficient estimates of linear probability regressions and ordinary least squares regressions. I regress two proxies of financial misconduct on the interaction of *Treated* and year indicators, which are centered around event years. *Treated* is an indicator variable that takes a value of 1 if a branch experiences the death of a minority advisor at any time during the sample period. *Event [-1]* represents the years preceding the event year by one year, while *Event [-2]* represents the years preceding the event year by two years. Similarly, *Event [+1]* represents the years following the event year by one year, and *Event [+2]* represents the years following the event year by two years. *Event [+3]* captures years that are three or more years after the event year. *Event [0]* represents the event years. *Misconduct dummy* is an indicator variable equal to one if the branch experiences a misconduct. *Ln(Misconduct)* represents the natural logarithm of the total number of misconducts within a branch. Variable definitions for controls are provided in the appendix. Column (1) and Column (4) include fixed effects for branch (firm-by-county) and year. Columns (2) and (5) further incorporate firm-by-year fixed effects, while Columns (3) and (6) include county-by-year fixed effects, respectively. Observations are branch-by-year. Standard errors are clustered at branch level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

	Misconduct Dummy			Ln(Misconduct)		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Event[-2] X Treated	-0.013	-0.019	-0.021	-0.017	-0.022	-0.025
	(-0.88)	(-1.13)	(-1.30)	(-1.22)	(-1.37)	(-1.49)
Event[-1] X Treated	0.011	0.018	0.011	0.003	0.006	0.001
	(0.70)	(1.01)	(0.58)	(0.20)	(0.34)	(0.05)
Event[0] X Treated	0.008	0.004	-0.007	0.043**	0.044**	0.036*
	(0.54)	(0.21)	(-0.37)	(2.31)	(2.14)	(1.69)
Event[1] X Treated	0.039**	0.043**	0.035*	0.093***	0.101***	0.092***
	(2.39)	(2.29)	(1.79)	(4.51)	(4.53)	(3.99)
Event[2] X Treated	0.048***	0.046**	0.035*	0.091***	0.093***	0.082***
	(2.84)	(2.40)	(1.77)	(4.28)	(4.06)	(3.47)
Event[3] X Treated	0.025**	0.027**	0.015	0.035**	0.041**	0.033*
	(2.24)	(2.32)	(1.17)	(2.15)	(2.56)	(1.92)
Constant	-0.153***	-0.079***	-0.081***	-0.214***	-0.112***	-0.119***
	(-13.66)	(-20.19)	(-19.07)	(-14.59)	(-18.05)	(-17.14)
Observations	465,891	465,891	465,891	465,891	465,891	465,891
R-squared	0.328	0.367	0.399	0.452	0.497	0.522
Branch FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Firm-Year FE	N	Y	Y	N	Y	Y
County-Year FE	N	N	Y	N	N	Y

Table 7 Split sample analyses

This table reports split analyses at advisor level (Panel A) and branch level (Panel B), respectively. Both panels report coefficient estimates of linear probability regressions. I regress misconduct indicator on *Branch Diversity*. *Branch Diversity* represents the branch diversity measured using the Herfindahl-Hirschman Index. Variable definitions for controls are provided in the appendix. Panel A presents split sample analyses on ethnicity, gender, and advisor experience. Panel B focuses on branch size, branch county demographic diversity, and firm historical misconduct rate. Continuous variables are split at the median level. All columns include branch (firm-by-county), firm-by-year, and county-by-year fixed effects. T-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

Panel A. Advisor-level

	Ethnicity		Gender		Advisor Ln(Experience).	
VARIABLES	White	Minority	Male	Female	>Median	<=Median
Branch Diversity	**-0.005*** **	0.001	**-0.005*** **	-0.002**	**-0.006*** **	-0.002**
	(-6.49)	(0.46)	**(-5.37)**	(-2.26)	**(-3.73)**	(-2.38)
Observations	11,971,109	1,255,433	9,845,424	3,363,256	3,886,763	9,337,006
R-squared	0.038	0.067	0.042	0.047	0.055	0.038
All Controls	Y	Y	Y	Y	Y	Y
Branch FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Firm-Year FE	Y	Y	Y	Y	Y	Y
County-Year FE	Y	Y	Y	Y	Y	Y
Wald Test	5.49**		7.71***		6.23**	

Panel B. Branch-level

	Branch Size		County Diversity		Firm Hist Misconduct	
VARIABLES	>Median	<=Median	>Median	<=Median	>Median	<=Median
Branch Diversity	-0.024	**-0.034*** **	**-0.054*** **	-0.023	**-0.067*** **	-0.025***
	(-1.54)	**(-6.91)**	**(-6.84)**	(-1.51)	**(-5.57)**	(-3.61)
Observations	203,882	232,992	258,172	198,784	220,853	224,540
R-squared	0.558	0.365	0.570	0.421	0.567	0.520
All Controls	Y	Y	Y	Y	Y	Y
Branch FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Firm-Year FE	Y	Y	Y	Y	Y	Y
County-Year FE	Y	Y	Y	Y	Y	Y
Wald Test	5.19**		5.47**		6.45**	

Table 8 Branch-level further analysis

This table reports coefficient estimates of linear probability regression and ordinary least squares regression. I regress *Branch Closure* and *Branch Turnover* on *Branch Diversity* for years *t+1*, *t+2*, and *t+3*, respectively. *Branch Closure* is an indicator variable set to one if the branch is terminated. *Branch Turnover* is the fraction of advisors leaving the branch. *Branch Diversity* represents the branch diversity measured using the Herfindahl-Hirschman Index. Variable definitions for branch and firm controls are provided in the appendix. All columns include branch (firm-by-county), year, firm-by-year fixed effects. In addition, county level control variables are also included. *Ln(Population)* represents the natural logarithm of the population of the county where the branch is located, along with the population change *Population Δ*. *County Diversity* refers to the HHI-based demographic diversity. *Unemployment* represents the unemployment rate, whereas *Median Income* and *Bachelor* represent household income and the fraction of individuals with a bachelor's degree. Additionally, the variable *Republican* is an indicator variable equal to 1 if the county is classified as republican. Observations are branch-by-year. Standard errors are clustered at branch level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

	Branch Closure			Branch Turnover		
	t+1	*t+2*	*t+3*	*t+1*	*t+2*	*t+3*
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Branch Diversity	-0.027***	-0.035***	-0.033***	-0.013**	-0.019***	-0.024***
	(-5.03)	(-6.48)	(-5.99)	(-2.18)	(-3.03)	(-3.59)
ln(Branch Size)	-0.031***	-0.022***	-0.020***	0.039***	0.032***	0.021***
	(-28.30)	(-19.83)	(-16.63)	(27.18)	(21.31)	(13.75)
Log(Population)	0.020***	0.007	0.010	0.007	0.002	0.001
	(2.58)	(0.88)	(1.21)	(0.76)	(0.19)	(0.14)
County Diversity	-0.107***	-0.093***	-0.078***	-0.079***	-0.067**	-0.045
	(-4.63)	(-3.91)	(-3.06)	(-2.87)	(-2.33)	(-1.48)
Population Δ	-0.305***	-0.342***	-0.285***	-0.289***	-0.307***	-0.290***
	(-5.53)	(-6.10)	(-5.19)	(-4.56)	(-4.75)	(-4.47)
Bachelor	-0.003***	-0.004***	-0.003***	-0.004***	-0.005***	-0.004***
	(-7.70)	(-8.17)	(-7.28)	(-7.58)	(-8.94)	(-8.28)
Unemployment	-0.000	0.000	0.000	0.000	0.000	-0.000
	(-0.31)	(0.54)	(0.12)	(0.84)	(0.44)	(-0.33)
Republican	0.146***	0.109***	0.117***	0.166***	0.148***	0.159***
	(12.29)	(9.12)	(9.46)	(12.67)	(10.99)	(11.36)
Median Income	-0.029***	-0.007	-0.006	-0.020**	0.000	-0.003
	(-3.61)	(-0.85)	(-0.66)	(-2.10)	(0.03)	(-0.26)
Constant	0.253**	0.175	0.094	0.246*	0.134	0.177
	(2.06)	(1.38)	(0.69)	(1.74)	(0.90)	(1.11)
Observations	349,510	306,479	271,879	349,510	306,479	271,879
R-squared	0.545	0.560	0.576	0.546	0.553	0.562
Branch FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Firm-Year FE	Y	Y	Y	Y	Y	Y

Table 9 Robustness with alternative measure

This table reports coefficient estimates of linear probability regressions. *Misconduct dummy* is an indicator variable equal to one if the branch experiences a misconduct. *Entropy* represents the entropy branch diversity. Variable definitions for controls are provided in the appendix. Column (1) includes branch (firm-by-county) and year fixed effects. Columns (2)-(3) further incorporate firm-by-year and county-by-year fixed effects, respectively. Observations are branch-by-year. Standard errors are clustered at branch level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels,53especttively.

	Misconduct Dummy		
VARIABLES	(1)	(2)	(3)
Entropy	-0.019***	-0.015***	-0.016***
	(-8.03)	(-5.34)	(-5.10)
Firm Hist Misconduct	0.794***		
	(21.62)		
ln(Branch Size)	0.092***	0.091***	0.092***
	(54.37)	(51.46)	(48.43)
ln(Firm Size)	0.011***		
	(5.93)		
ln(Firm Age)	-0.010***		
	(-3.97)		
Constant	-0.155***	-0.080***	-0.081***
	(-13.84)	(-20.71)	(-19.36)
Observations	465,891	465,891	465,891
R-squared	0.327	0.366	0.399
Branch FE	Y	Y	Y
Year FE	Y	Y	Y
Firm-Year FE	N	Y	Y
County-Year FE	N	N	Y

Table 10 Robustness with Ethnicolr algorithm

This table reports coefficient estimates of linear probability regressions. *Misconduct dummy* is an indicator variable equal to one if the branch experiences a misconduct. *Ethnicolr Diversity* represents the branch diversity constructed based on Ethnicolr name-classifier. Variable definitions for controls are provided in the appendix. Column (1) includes branch (firm-by-county) and year fixed effects. Columns (2)-(3) further incorporate firm-by-year and county-by-year fixed effects, respectively. Observations are branch-by-year. Standard errors are clustered at branch level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

	Misconduct Dummy		
VARIABLES	(1)	(2)	(3)
Ethnicolr Diversity	-0.035***	-0.030***	-0.030***
	(-6.00)	(-4.30)	(-3.93)
Firm Hist Misconduct	0.794***		
	(21.60)		
ln(Branch Size)	0.091***	0.091***	0.092***
	(54.06)	(51.30)	(48.27)
ln(Firm Size)	0.011***		
	(5.93)		
ln(Firm Age)	-0.010***		
	(-3.95)		
Constant	-0.138***	-0.065***	-0.067***
	(-12.06)	(-12.89)	(-12.08)
Observations	465,891	465,891	465,891
R-squared	0.327	0.366	0.399
Branch FE	Y	Y	Y
Year FE	Y	Y	Y
Firm-Year FE	N	Y	Y
County-Year FE	N	N	Y

Table 11 Robustness with advisor FEs

This table reports coefficient estimates of linear probability regressions. Misconduct dummy is an indicator variable equal to one if the advisor commits a misconduct. Branch Diversity represents the branch diversity measured using the Herfindahl-Hirschman Index. Variable definitions for controls are provided in the appendix. Column (1) includes branch (firm-by-county), year, and advisor fixed effects. Columns (2)-(3) further incorporate firm-by-year and county-by-year fixed effects, respectively. Observations are advisor-by-year. Standard errors are clustered at firm level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

	Misconduct Dummy		
VARIABLES	(1)	(2)	(3)
Branch Diversity	-0.005***	-0.003**	-0.005***
	(-4.97)	(-2.46)	(-5.19)
Observations	13,186,592	13,186,592	13,186,592
R-squared	0.143	0.152	0.156
All Controls	Y	Y	Y
Branch FE	Y	Y	Y
Year FE	Y	Y	Y
Firm-Year FE	N	Y	Y
County-Year FE	N	N	Y
Advisor FE	Y	Y	Y

Table 12 Robustness with potential survivorship bias

This table reports coefficient estimates of linear probability regressions over the 2010-2020 sample period. *Misconduct dummy* is an indicator variable equal to one if the branch experiences a misconduct. *Branch Diversity* represents the branch diversity measured using the Herfindahl-Hirschman Index. Variable definitions for controls are provided in the appendix. Column (1) includes branch (firm-by-county) and year fixed effects. Columns (2)-(3) further incorporate firm-by-year and county-by-year fixed effects, respectively. Observations are branch-by-year. Standard errors are clustered at branch level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

	Misconduct Dummy		
VARIABLES	(1)	(2)	(3)
Branch Diversity	-0.042***	-0.044***	-0.037***
	(-6.35)	(-5.20)	(-3.96)
Firm Hist Misconduct	0.565***		
	(10.52)		
ln(Branch Size)	0.068***	0.068***	0.069***
	(30.96)	(28.96)	(27.37)
ln(Firm Size)	0.008***		
	(2.80)		
ln(Firm Age)	-0.005		
	(-1.44)		
Constant	-0.098***	-0.037***	-0.039***
	(-5.84)	(-7.13)	(-6.87)
Observations	260,212	260,212	260,212
R-squared	0.356	0.386	0.415
Branch FE	Y	Y	Y
Year FE	Y	Y	Y
Firm-Year FE	N	Y	Y
County-Year FE	N	N	Y

Table 13 Stacked difference-in-difference analysis

This table presents coefficient estimates obtained from linear probability regressions and ordinary least squares regressions conducted on three subsamples of matched treatment-control branch pairs. I regress two proxies of financial misconduct on the interaction of *Treated* and *After*. *Treated* is an indicator variable that takes a value of 1 if a branch experiences the death of a minority advisor at any time during the sample period. *After* takes a value of 1 for branch-year observations occurring after the event. *Misconduct dummy* is an indicator variable equal to one if the branch experiences a misconduct. *Ln(Misconduct)* represents the natural logarithm of the total number of misconducts within a branch. Variable definitions for controls are provided in the appendix. Column (1) and Column (4) present results for matched pairs within the same firm. Columns (2) and (5) show results for matched pairs within the same county, while Columns (3) and (6) display results for matched pairs within the same firm and state. Observations are branch-by-year-by-pair. Standard errors are clustered at branch level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

	Misconduct Dummy			Ln(Misconduct)		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Treated X After	0.085***	0.036***	0.079***	0.133***	0.097***	0.150***
	(5.73)	(3.35)	(4.84)	(8.65)	(6.45)	(6.31)
Observations	663,938	702,290	37,108	663,938	702,290	37,108
R-squared	0.750	0.766	0.763	0.827	0.849	0.849
All Controls	Y	Y	Y	Y	Y	Y
Branch-Pair FE	Y	Y	Y	Y	Y	Y
Year-Pair FE	Y	Y	Y	Y	Y	Y

Table 14 Stacked difference-in-difference treatment dynamics

This table reports coefficient estimates of linear probability regressions. I regress misconduct indicator on the interaction of *Treated* and year indicators, which are centered around event years. *Treated* is an indicator variable that takes a value of 1 if a branch experiences the death of a minority advisor at any time during the sample period. *Event [-1]* represents the years preceding the event year by one year, while *Event [-2]* represents the years preceding the event year by two years. Similarly, *Event [+1]* represents the years following the event year by one year, and *Event [+2]* represents the years following the event year by two years. *Event [+3]* captures years that are three or more years after the event year. *Event [0]* represents the event years. *Misconduct dummy* is an indicator variable equal to one if the branch experiences a misconduct. Variable definitions for controls are provided in the appendix. Column (1) presents results for matched pairs within the same firm. Column (2) shows results for matched pairs within the same county. Column (3) displays results for matched pairs within the same firm and state. Observations are branch-by-year-by-pair. Standard errors are clustered at branch level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

VARIABLES	Same Firm-Year Misconduct Dummy	Same County-Year Misconduct Dummy	Same Firm-State-Year Misconduct Dummy
Event[-2] X Treated	-0.031	-0.019	-0.031
	(-1.33)	(-1.06)	(-1.04)
Event[-1] X Treated	0.021	-0.011	-0.004
	(0.85)	(-0.59)	(-0.14)
Event[0] X Treated	0.047	0.009	-0.020
	(1.61)	(0.46)	(-0.64)
Event[+1] X Treated	0.124***	0.043**	0.090***
	(5.21)	(2.35)	(2.84)
Event[+2] X Treated	0.079***	0.028	0.040
	(3.17)	(1.46)	(1.27)
Event[+3] X Treated	0.087***	0.016	0.058*
	(3.23)	(0.82)	(1.85)
Constant	-0.200***	-0.189**	-0.187***
	(-5.64)	(-2.26)	(-3.59)
Observations	663,938	702,290	37,108
R-squared	0.751	0.766	0.763
All Controls	Y	Y	Y
Branch-Pair FE	Y	Y	Y
Year-Pair FE	Y	Y	Y

CHAPTER 3

MIGRATION FEAR AND MINORITY CROWD-FUNDING SUCCESS: EVIDENCE FROM KICKSTARTER

Related Literature and Hypothesis Development

This chapter builds upon and contributes to two literatures: 1) the evidence on biases against minorities in entrepreneurial finance and on crowd-funding sites and 2) studies of how migration fear builds upon and exacerbates biases against minorities.

Biases in Entrepreneurial Finance and Crowd-Funding Platforms

A significant literature documents the persistent and severe challenges that minorities face in raising entrepreneurial and business finance. Blanchflower et al. (2003) measure Black-owned small businesses are about twice as likely to be denied credit as non-minorities, and Blanchard et al. (2008) further identify discrimination for Hispanic-owned businesses. Multiple studies quantifying racial differences in self-employment measure the important role of capital access (Fairlie and Robb 2007), including recent work by Hamilton et al. (2022) and Fairlie et al (2022). These differences in access to financial capital can preclude individuals from entering new businesses and projects altogether or require them to start at a suboptimal size (Evans and Jovanovic 1989, Hurst and Pugsley 2018). More recently, Howell et al. (2021) and Chernenko and Scharfstein (2022) document how Black-owned businesses were less likely during the Covid-19 pandemic to obtain a Paycheck Protection Program loan and instead utilize fintechs.[14]

A complementary literature examines the rise of crowd funding, following upon Mollick (2014). A prominent hope of crowd funding is that it will “democratize” access to finance (e.g., Agrawal et al. 2014, Mollick and Robb 2016, Younkin and Kashkooli 2016), and to some degree crowd funding has weakened the “home bias” for investment in local geographic areas (e.g., Agrawal et al. 2011, 2015, Kim and Hann 2014, Lin and Viswanathan 2016). Social capital plays an important role in raising support among backers (Manikandan 2020). Many projects on Kickstarter relate to the launch of a business to support the creative effort, with Mollick and Kuppuswamy (2014) exploring in-depth the creation of gaming ventures following a successful crowd-funding campaign. Crowd funding allows early validation of product demand and may be a path for individuals with lower risk tolerances to test ideas and enter (Hvide and Panos 2014).

Yet, several studies establish that racial and gender biases have carried over to crowd funding. The closest research to this study is Younkin and Kuppuswamy (2018), who document a baseline racism on Kickstarter against Black creators due to unconscious bias. These authors argue this bias can be lowered through endorsements, prior success, and removing race indicators like photos. Gorbatai et al. (2021) further quantify how crowd-funding behavior changes in the immediate aftermath of salient events, such as police shootings exacerbating racial biases again Black creators on Kickstarter. Additionally, several studies consider gender differences (Ewens and Townsend 2019). In a rare study with data on backers of crowd-funding projects, Gafni et al. (2021) find evidence for taste-based

[14] See also Asiedu et al. (2012), Bayer et al. (2018), Begley and Purnanandam (2021), and Cassel et al. (2021). Robb and Robinson (2014) provide an overview of start-up capital. Kerr et al. (2014) establish the importance early stage financing can have for venture success.

discrimination along gender lines on Kickstarter. In an equity crowd-funding setting, Bapna and Ganco (2021) find that gender bias is strongest in low-stakes settings, with no bias uncovered in high-stakes settings.

I contribute to this literature by studying the ability of minority creators to successfully raise their funds during different social and political environments. Building on studies that quantify discrimination against minorities at points in time, I analyze quarterly variation in sentiment and minority campaign success. The findings of this study, which are of significant economic magnitude as well as precisely estimated, thus shed light on how biases in entrepreneurial finance form and are exacerbated temporarily by the surrounding social and political environment. Moreover, as my estimation framework isolates high-frequency variation for identification, this study is an important complement to studies that estimate race effects in financial settings and then seek to parse the role of biases and discrimination vs. other factors that might result in racial differences.

Migration Fear and Biases Towards Minorities

While immigrants account for over 14% of the U.S. population in 2022, public attitudes regarding migration have spiked in hostile sentiment throughout the nation's history. Ending the relatively open border period during the Age of Mass Migration (Abramitzky and Boustan 2022), the Immigration Act of 1924 severely limited immigration to America from places outside of selected countries in northwestern Europe and was partly motivated by politicians as necessary to protect the nation's racial purity (e.g., Doran and Yoon 2019, Moser et al. 2019). While the Immigration and Nationality Act of 1965 later loosened policy and allowed for a more diverse set of origin countries, Goodman (2020) describes in *Deportation Machine* the many instances of local hostility spiking towards

Chinese and Mexican immigrants as they became more prominent and the long history of politicians decrying immigration when rallying support to their campaigns.

The public often carries misperceptions about immigrants and long-term acceptance and assimilation of immigrants takes time (e.g., Card et al. 2005, Clemens 2011, Weber 2019, Bursztyn et al. 2021, Alesina et al 2022). While scholars note multiple causes for the formation of periodic hostile public attitudes towards immigration,[15] research consistently shows that public discourse and opinions on immigration are closely intertwined with attitudes toward racial and ethnic groups.[16] For example, Hartman et al. (2014) provide evidence that white Americans are significantly more offended by norm violations, such as entering the country illegally or working off the books, for Hispanics than for white Europeans. This work concludes hostility towards immigrants is largely social and psychological in nature, whereby prejudice, stereotyping, and group-based biases against minority ethnic groups often play an important role (Kinder and Kam 2010, Hainmueller and Hopkins 2014).

During the last decade, this academic work has held renewed importance, as public concern over immigration and national identity spiked during key moments of the campaign and administration of President Trump, as well as outside the United States. While the public rhetoric often focused on immigration, such the building of the wall on the southern border to Mexico, hostile reactions can engulf a broader set of citizen minorities as well. The rise of white nationalist movements and their infusion into U.S. politics during the last decade came with immigration at the heart of political messaging, including concepts like “replacement

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[15] For example, Tichenor (2002), Arzheimer (2009), Dancygier (2010), Lahav and Courtemanche (2012), and Hainmueller and Hopkins (2014).

[16] For example, Nelson and Kinder (1996), Citrin et al. (1997), King (2000), Kinder (2003), and Law and Zou (2021). Recent research has further explored boundaries that develop between minority groups, like Fouka and Tabellini (2021), Fouka et al (2022), and Cikara et al. (2022). Borjas et al. (2006) consider competition in the labor market.

theory" of a white majority through higher levels of immigration and the linking immigrants to crime (Clark 2020).[17] Several studies quantify the propagation of hostile attitudes evoked by President Trump towards minorities through social media (e.g., Edwards and Rushin 2019, Bursztyn et al. 2020, Newman et al. 2020, Müller and Schwarz 2022, Cao et al. 2022). Additional work identifies localized effects like higher racial profiling in police traffic stops in counties after a Trump political rally (Grosjean et al. 2022).

These hostile settings can impact economic outcomes. From a historical perspective, Cook (2014) documents how fear in the Jim Crow era reduced Black innovation. More recently, Kang (2020) documents that minority CEOs exhibit more pessimistic earnings forecasts after Trump's election. They also express more concerns about litigation and migration risk. Doleac and Stein (2013) measure lower trust for minorities in online settings with experiments, which could be exacerbated with hostile public opinion or uncertainty.

Data, Sample, and Methodology

Data Source: Kickstarter.com

My main data source comes from Kickstarter.com, which is one of the largest unregulated crowd-funding platforms. On Kickstarter, creators and entrepreneurs seeking capital to complete a specific "creative" project disclose their plans and funding needs via a web page that contains a main body (comprised of video, images, and text), funding status, and reward tiers. Since inception, Kickstarter has enjoyed wide popularity, and by 2022, more than 21 million people have backed a project and the total dollars pledged to

[17] In January 2018, President Trump criticized immigration from "sh##hole" countries with reference to Haiti and Africa as well as some nations in Latin America. President Trump's contrasting favoring of immigration from Norway at this time was widely interpreted as being along racial lines against Blacks and Hispanics.

Kickstarter projects has exceeded $7 billion.[18] Projects on Kickstarter are grouped into 15 broad categories: Art, Comics, Crafts, Dance, Design, Fashion, Film & Video, Food, Games, Journalism, Music, Photography, Publishing, Technology, and Theater.

In exchange for monetary pledges, creators make nonbinding and unenforceable promises to deliver "rewards" that are often in the form of finished products, early-stage prototypes, or early access to certain services in the future (e.g., Krishnan et al. 2017). To illustrate, contributors to the launch of a bakery might receive a baked loaf of bread or a one-on-one cooking class with the chef depending upon the size of financial support. If the sum of pledges received during the funding period—between one and 60 days, with 30 days as the most common project duration—meets or exceeds the funding goal, then the project is funded. Otherwise, all pledges are returned to the backers (i.e., Kickstarter provides "all or nothing" funding).[19]

Figure 3 provides an example project from Kickstarter. The project raised funds for a debut album from Joey Garcia, entitled Woke Up Running. As of March 2022, the drive had raised $4095 against an initial goal of $3700. Pledges of $20 or more received a signed hard copy of the album and a JG sticker, while pledges of $1600 or more received several rewards, including Garcia writing and recording a song "about you or for you." Garcia lives in Plymouth, IN, and his bio reads: "I'm small-town people trying to do something I love doing for a living. It's a dream that most fail but I refuse to give up. I have a full time job but music is my life. I've fallen to the bottom but just get right back up." Twelve of Garcia's 92 backers came from Plymouth, while three backers were outside of the United States. Garcia

[18] Statistics retrieved from https://www.kickstarter.com/help/stats (December 2022).

[19] This differs from GoFundMe and Indiegogo, neither of which is "all or nothing" funding. Kickstarter focuses on creative projects in art, music, film, etc. (e.g., Mollick and Nanda 2016). GoFundMe is usually for individuals and personal causes. Indiegogo accommodates many diverse campaigns.

posted 7 updates during the course of the campaign.

Sample Selection

To download comprehensive data on Kickstarter projects, I use https://webrobots.io, which is a web crawling company that extracts information from public websites. I download Kickstarter datasets starting from January 2016, and these downloads contain retrospective project information tracing back to Kickstarter's initial launch on April 28, 2009.[20] These data include information about project creators, number of project backers, project descriptions, locations, goals (target amount to be raised), pledges (the amount that has been donated), whether projects are classified under "Staff Picked" category, and project launch dates and deadlines.

From this initial downloaded data, I drop suspended projects and projects with missing text or incomplete creator name fields. In 7.3% of cases, the creator's name field contains only non-name elements (e.g., "Dark Elf's Games", "Vibrant Sounds"); an additional 0.5% of projects combine a name with non-name elements ("Saxophonist Ted Allen", "Carole's Candy Shop"). To identify these cases, I manually reviewed all creator names to flag non-name elements. I drop these cases from my estimations, and my results are robust to including cases with some name-related elements or including all projects with additional interactions for non-name entities. Finally, to increase the precision of minority status of creators, I drop a small share of projects with more than one creator. My analyses focus on projects located in the United States.

Variable Construction

The main variable of interest is whether a creator is a U.S. ethnic minority. To infer

[20] Starting from December 2015, Webrobots' web scraping algorithm collected all the sub-categories of Kickstarter data, giving a comprehensive view of projects. Since March 2016, the scraping frequency has been monthly. See https://webrobots.io/kickstarter-datasets/ and https://webrobots.io/about-us/.

creators' racial ethnicity, I primarily use NamePrism (https://www.name-prism.com/), which is a nationality and ethnicity classification tool based on name embeddings created by Ye et al. (2017) and Ye and Skiena (2019). NamePrism makes available their APIs for academic and non-commercial purposes, supporting hundreds of research projects. In parallel, prominent work using names to identify or signal minority status includes Bertrand and Mullainathan (2004), Fryer and Levitt (2004), and Kline et al. (2021).

NamePrism's algorithm uses a naïve Bayes model which depends on first and last names, inferring ethnicity for six categories: White; Black; Hispanic; Asian and Pacific Islander (API, hereafter Asian); American Indian and Alaska Native (AIAN); and More than Two Race (2PRACE). The API returns a dictionary of six ethnicity categories with their respective probabilities. After extracting the inferred probabilities of Kickstarter creators, I create an indicator variable *Minority* which is equal to one if the highest inferred probability belongs to a Black, Hispanic, or Asian category, and zero otherwise. Robustness checks will also consider the cumulative probability that the project creator is non-white.[21] I later discuss robustness checks that utilize two other name classification algorithms and also a method that uses visual pictures of creators.

The main explanatory variable is the Migration Fear Index first developed by Baker et al. (2015, 2016). The index counts the number of newspaper articles with at least one term from each of the Migration and Fear term sets, and then dividing by the total count of newspaper articles (in the same calendar quarter and country). The Migration word list includes "border control", "open borders", migrant, migration, asylum, refugee, immigrant, immigration, assimilation, Schengen, and "human trafficking", while the Fear terms include

[21] There are very few AIAN and 2PRACE cases (collectively summing to 0.03% of sample), which I leave in the baseline category with white creators when modelling indicator variables for minorities.

anxiety, panic, bomb, fear, crime, terror, worry, concern, and violent. For presentation purposes, I divide the raw index number by 100.

I use three measures of project funding success. Given the "all or nothing" nature of Kickstarter's fund-raising structure, the main dependent variable is an indicator variable *Success*, which is equal to one if a project has reached to its funding target, and zero otherwise. The second outcome variable *Pledges/Goal* is equal to the total amount of dollars pledged to the project, scaled by the project's initial goal. This measure can exceed a value of one, as projects can be oversubscribed and some creators leave projects open to continue outreach. I thus cap the ratio at 125% of goal, with about 20% of the sample at this top coded value. Finally, ln(*Backers)* is equal to the log number of project backers.

Summary Statistics

Table 15 presents summary statistics for key variables used in my analysis, with Appendix table providing additional details on variable construction. After cleaning the Kickstarter data and merging it with the Migration Fear Index, I have 150,282 project observations between 2009q2 and 2021q1 for my core analysis. The average success rate of projects is 48.8%, while the average project in my sample has a pledged amount equivalent to 60.2% of its funding goal, with the max of 125%. The mean number of backers is 78.6. Minority creators account for 9.3% of projects, which is significantly less than their share of the U.S. population, and the sample is also heavily skewed toward men. The Black, Hispanic, and Asian creator shares are 1.1%, 5.1%, and 3.2%, respectively, in my main sample.

Figure 4 shows two features of the Kickstarter data that influence my estimation design. First, minority creators have been a slowly but steadily growing part of the Kickstarter platform, rising from around 7% in 2009 to typically 10%-11% in recent years.

Overall, this steady growth suggests a limited role for the extensive margin in terms of minority creators being differentially likely to post projects. I thus focus on rates of success for posted projects in my analysis, with a matched sample exercise to complement. Second, from a small initial start, the number of projects listed on Kickstarter grew to peak in 2014-2015, subsequently diminishing steadily over time. This peak period and its reversion were accompanied by a macro dip in the general rate of success for projects by creators of all ethnic groups. I show this peak period does not influence the estimates in robustness checks.

Results

Visual Series

Before using regression models, I commence with simple visual evidence in Figure 1. The red dashed line is the Migration Fear Index first developed by Baker et al. (2015, 2016), divided by 100 and with values shown on the right-hand axis. The horizontal axis documents quarters, and I have marked several significant events on the figure.

From the start of the sample to early 2015, the Migration Fear Index for the United States was typically under a value of one, with very modest fluctuations. Starting in 2015, contemporaneous with Donald Trump's campaign launch, the Migration Fear Index begins to reach double its baseline level. The index shows modest further growth alongside Trump's continued success to win the Republican nomination. The sharp downward dip in the index in 2016q2 coincides with a relatively quiet quarter after Trump and Hillary Clinton have secured their respective party nominations but before the summer conventions and the fall general election begins in earnest. The key Brexit vote in the United Kingdom also falls in June 2016.

Upon President's Trump's surprise win in November 2016, the Migration Fear Index moves further higher as he takes office and enacts Executive Orders 13769 and 13780, the policies often termed the "Muslim travel ban," placing strict limits on travel to the United States by nationals of several countries and barring entry for refugees who did not possess visas or valid travel documents.[22] At its peak in 2017q3, corresponding to the summer of Trump's first year in office and including events like the Unite the Right rally in Charlottesville, VA (August 11-12), the Migration Fear Index measured more than triple its initial level. The index's rebound in 2018 coincides with Trump's criticism of immigration from "sh##hole" countries (see footnote 4), the announcement of "zero tolerance" policies on the border that included family separations, and even unconstitutional threats to revoke birthright citizenship. The Migration Fear Index would remain elevated through most of Trump's presidency with further declines as Joe Biden wins the 2020 election and takes office.

The solid green line with circle markers in Figure 1 shows the success difference of white- vs. minority-created projects on Kickstarter scaled by the total rate of project success: (success rate for white creators – success rate for minority creators) / (total average success rate). Values for this series are shown on the left-hand axis, with positive values indicating white creators are achieving their funding goals at a higher rate than minorities. In most every quarter, white creators are more likely to reach funding goals than minority creators. For most quarters prior to 2015, the differential is 10% or less. Commencing in 2015, however, the differential is rarely less than 20% until 2021. What is also visually remarkable is the quarterly overlay of the two series. While the co-movements are not perfectly

[22] While these executive orders primarily concern countries of Muslim religion, they resulted in significant concern and uncertainty regarding U.S. immigration policy. See for example, https://www.washingtonpost.com/news/the-fix/wp/2017/01/29/president-trumps-travel-ban-is-causing-chaos-dont-expect-him-to-back-down/;https://www.aljazeera.com/news/2020/2/2/trumps-expanded-travel-ban-sows-fear-in-communities-across-us;https://www.wsj.com/articles/nothing-redeems-trumps-travel-ban-1485907087.

synchronized, there is a tight linkage that visually foreshadows the strength of regressions conducted in the next subsection.

Finally, Figure 1 shows a subsequent spike of success differentials in 2020 after the outbreak of the Covid-19 pandemic. This spike is less timed to the Migration Fear Index, and I show in the Appendix the link of widening gap to challenges encountered by Chinese creators during the worst periods of Asian Hate.[23]

Baseline Results

Building on Figure 1, regression analyses examine how funding success for minority creators differs across levels of the Migration Fear Index. I use the following model:

$$\begin{aligned} Funding\ outcome_i \\ &= \beta_1 Minority_i + \beta_2 Minority_i \times Fear_{tq} + \beta_3 Fear_{tq} \\ &+ \Omega\, Controls_i + \lambda\, Fixed\ Effects_{it} + \varepsilon_i \end{aligned} \tag{1}$$

where i, q and t index projects, quarters and years, respectively. $Minority_i$ is an indicator variable that is equal to one if a creator is a minority, and zero otherwise. $Fear_{tq}$ is the Migration Fear Index for quarter q and year t. I include fixed effects for the project category and state location of the creator to account for time-invariant factors that influence typical rates of funding success. I also include year fixed effects to account for nationwide factors such as macroeconomic conditions that impact funding outcomes. With year fixed effects, I only use the quarterly variation in the Migration Fear Index for identification. The beta coefficients thus do not reflect the large-scale shift depicted in Figure 1 and described in my visual analysis, but instead only quantify localized variation within calendar years.

The vector of control variables includes other observable project- and creator-level

[23]Figure 5 shows Figure 1 with raw funding success rates for white and minority creators. Figure 1's differential, scaled metric abstracts from macro shifts in project success rates common to all creators.

characteristics that may impact funding success. I control for the project goal (ln*(Goal)*) defined as target funding amount; the project horizon (ln*(Horizon)*) calculated as the number of days between project deadline and its initial launch date; the total number of projects (ln*(Total Projects)*) launched in same year-quarter with project *i* by the same creator; and project length (ln*(Length)*) computed as the length of project description. Following Gafni et al. (2019), I also control for self-mentions by including an indicator variable for when project creators mention their first or last name in the project description. In addition, I include a *Female* creator indicator (Gafni et al. 2021). Reflecting the two sources of variation in the key interaction term $Minority_i \times Fear_{tq}$, I two-way cluster standard errors by creator and quarter, reporting t-statistics in the parenthesis. Similarly, I weight regressions so that each creator-quarter carries the same importance (i.e., the weight is 1 divided by the number of projects sponsored by the creator in a given quarter).

Panel A of Table 16 reports the baseline results of estimating Equation (1). In Column (1), higher values of the Migration Fear Index are statistically linked with lower rates of project success for minorities compared to white creators. With a coefficient of -0.0321, I estimate a one standard-deviation increase in the Migration Fear Index (equal to 0.601) translates to a decline of 1.93% in the likelihood of minority projects being successfully funded compared to a baseline average of 48.8% (thus, a relative effect of about 3.9% compared to the baseline). This decline in minority success with a one standard-deviation increase in the index is comparable in size to the baseline gap of -2.1% measured with the main effect of $Minority_i$. The main effect of the Migration Fear Index itself is weak, indicating limited change in the likelihood of project success for white creators.

Columns (2) and (3) consider the dollar share of pledges achieved and the logarithm of the number of backers, respectively. While these alternative metrics capture different aspects of project success, my results consistently show that the crowd-funding efforts of minority creators are less fruitful in quarters when the Migration Fear Index is elevated. The magnitudes remain sizeable: a one standard-deviation increase in Migration Fear Index connects with a 2.3% decrease in achieved funding share compared to initial goal and an 8.8% decrease in the log number of project backers. In raw counts, the decrease is 19.0 backers (t-stat=-5.13), although the magnitude of this estimate depends upon how one treats very high backer counts.

Panel B parses the minority creator variable into specific ones for Black, Hispanic, and Asian creators based upon the most likely ethnicity/race for an individual. This empirical specification continues to measure coefficients relative to projects developed by white creators. This separation shows that funding shortfalls are evident across three minority groups across the 2009-2021 period. Black creators face the lowest main effects, with a 11.4% lower likelihood of successfully raising sought after funds, and show marginal evidence of further declines when the Migration Fear Index is high. Hispanic creators have a smaller main effect, with a 4.5% lower likelihood of successfully raising sought funds at baseline, but Hispanic creators experience the most deterioration during periods when the index is high. Finally, Asian creators have positive main effects, indicating more likely funding success than white creators absent migration fear, but this advantage also declines when the Migration Fear Index is elevated.

Table 18 presents a modified regression to test for non-linear effects and provide an additional interpretation for the magnitudes. I remove the linear interaction of *Minority x Fear* and instead introduce indicators for *Minority x Fear Medium* and *Minority x Fear High*, where I define *Fear Medium* and *Fear High* to be a Migration Fear Index value between [0.8, 1.75) and [1.75, max], respectively. These chosen points of 0.8 and 1.75 correspond to approximately the 25th and 75th percentiles, respectively, of the index from Table 15. The main effect on *Minority* captures the likelihood of a funding shortfall when the Migration Fear Index is low, estimating a 2.4% lower success rate. As the index rises, this minority gap triples to 8.2%, adding together the main effect and the interaction term. If I instead model indicator variables for the main effects of *Fear* as well (rather than keeping the linear term), I estimate a growth from a 3.9% lower probability of funding success for a minority creator at baseline to a 9.0% gap when the Migration Fear Index is in the top quartile of values. These swings represent large declines in funding success occurring within calendar years and conditional on controls.

Taking stock, the patterns in Tables 16-18 suggest minority creators experience more difficult crowd funding when concern regarding migration is high as measured by national media. While Black creators have the largest baseline funding shortfall, the fund raising of Hispanic creators exhibited the sharpest deterioration. This would align with much of the variation coming from the campaign and administration of President Trump, which focused most directly and explicitly on Hispanic migration. Black and Asian creators also show some decline that likely embodies direct effects, such as the “Chinese Virus” attacks examined later, with indirect spillovers from general migration concern and the link of immigration rhetoric to broader racist movements.

Robustness Checks and Identification Exercises

Table 19 provides several basic robustness checks about the use of the Migration Fear Index from Baker et al. (2015, 2016). Each row corresponds to a separate estimation where I report the focal interaction term for *Migration x Fear* and, in a few cases, the comparable interaction term introduced. In all cases where I add a potential explanatory variable, I include both a main effect for the added variable (not reported) and an interaction term with minority creators (reported). The sample size may change modestly if a variable's series ends before the baseline Migration Fear Index in 2021q1. All other regression details remain the same as in Column (1) of Table 16, with the key baseline estimation repeated in Row A.

Rows B-D start by contrasting the Migration Fear Index with other measures of economic and policy uncertainty, to ensure that my focus on the Migration Fear Index is not capturing a broader uncertainty beyond migration fear. I include from Baker et al. (2015, 2016) the Economic Policy Uncertainty Index in Row B and the Economic Policy Uncertainty Index (News) in Row C. In Row D, I include an Uncertainty Index based upon Twitter. While these added measures show some negative coefficients, they do not diminish the main interaction.

Row E next turns to a pre-post analysis of the surprising election of Trump in November 2016, as he was predicted by most forecasters to lose the election to Clinton. I restrict the sample period to 2015q4 – 2017q4 and replace the *Fear* index with an indicator variable *After*, which is set equal to one for the 4 quarters in year 2017. Row E reports the key interaction term that captures the differential change in funding outcomes between minority and white creators in 2017 compared to the period right before. Minority creators experienced a decrease in funding success probability in this analysis after the election.

Row F provides an alternative to the design of the Baker et al. (2015, 2016) indices, created using Google Search Values (Law and Zuo 2021). The news media in the United States has biases (Groseclose and Milyo 2005), and this extension helps confirm my findings are not particular to this metric's design. This extension confirms the baseline test, and I continue to prioritize the Baker et al. (2015, 2016) indices for my explanatory regressors given their independent construction.

Rows G-I compare the Migration Fear Index in the United States to those of the United Kingdom, Germany, and France. As highlighted in the introduction, the backlash against migration is not exclusive to the United States. Immigration was a key factor in Brexit voting, and migrant concerns (e.g., refugees) have spiked in Germany and France. These political movements are correlated across countries, and nations also report upon each other's news, leading to a macro index correlation above 0.5 across the four nations. Yet, due to the quarterly variation that I can isolate, the link to the U.S. Migration Fear Index strongly prevails over alternatives.

Finally, In Row J, I include a Bartik-style control for expected racial success that combines the distribution of minority projects across detailed product categories prior to 2013 with realized success rate by category in subsequent years. This specification continues to show a strong interaction term for minority creators and the fear index.

Table 20 shows additional robustness checks and extensions on the design. Row B shows similar results when excluding all project- and creator-level controls, and Row C shows very similar results when interacting all controls with the Migration Fear Index. Rows D and E show similar results when excluding sample weights or weighting estimates such that each creator receives the same overall weight, respectively. Rows F and G show robustness to incorporating city-year and product category-year fixed effects, respectively.

Kickstarter has a "Staff Picked" designation for about 10% of projects which raises their visibility to potential backers. Repeating specification (1) with a (0,1) indicator for being Staff Picked as the dependent variable yields a main effect for $Minority_i$ of 0.0037 (t-stat=0.56) and an interaction effect for $Minority_i \times Fear_{tq}$ of -0.0094 (t-stat=-2.25). In other words, while the projects of minority creators are equally likely to be Staff Picked as those of white creators in quarters when the index is low, minorities are less likely to be Staff Picked when the Migration Fear Index is high. This difference is quite likely due to the Staff Picked designation capturing backer momentum for projects that is not emerging for minority projects when concern is elevated. To ensure this exposure is not driving my results, Row H shows similar outcomes when excluding Staff Picked projects.

Row I delivers very similar outcomes when excluding the Kickstarter spike period of 2014q2 – 2015q3.

The last three rows of Table 20 show alternative ways to define racial minorities. Following Law and Zuo (2021), I first report in Row J results using the cumulative probability that the project creator is non-white, rather than a binary variable. Row K continues with an alternative ethnicity classifier Ethnicolr, developed by Ambekar et al. (2009), that uses deep learning techniques to classify names into ethnic groups trained on a Census Bureau dataset about the racial distribution of last names. These techniques deliver similar results.

Finally, for about 46% my sample [n=72,854], I have a low-resolution picture of the creator posted on Kickstarter. Using machine learning algorithms that designate ethnicity from pictures, I create an alternative code for minority status. I find quite similar results in Row L when using this approach, which is very comforting for my primary effort using name-based algorithms.[24]

[24] Tables 27 and 28 provide full results similar to Tables 16 and 17 for the picture-based sample. The results are quite comparable, with a notable difference being a stronger interaction of the Black creator

I finally build two auxiliary samples. Table 21 shows results with a matched sample that identifies pairs of similar projects that were listed on Kickstarter within 12 months of each other but during low vs. high levels of the Migration Fear Index. These matched projects are all sourced from 2015-2017 due the extreme fluctuations during those years.[25] Panel A considers the matched pairs for the projects created by minorities, and I estimate a one standard-deviation increase in the Migration Fear Index corresponds to a 1.6% lower likelihood of success. In Panel B, I do not observe a similar effect among matched projects for white creators. These results provide additional confidence that comparable projects of minorities are experiencing differential outcomes based upon conditions.

Table 22 examines creators that have developed multiple projects on Kickstarter. I include in the sample 40,729 projects from 14,722 creators with two or more projects. I keep the estimating equation as in specification (1) but further add creator fixed effects. With the creator fixed effect, I no longer estimate a minority main effect, but I can estimate the interaction term. I observe similar outcomes in this panel setting, with a relative effect of 2.7% in Column 1 that can be compared to 3.9% in the full sample of Table 16. While I favor estimations that allow for the single-time creators that are most common Kickstarter, the intensive margin shows a comparable magnitude.

Analysis of Backers and Mechanisms

Previous section demonstrates a strong empirical decline in funding success for minorities when the Migration Fear Index is high. This section next builds evidence on backers and variations in retractions of project support to shed light on mechanisms.

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dummy and the Migration Fear Index. I also find similar results when combining name and picture techniques. There is no strong time trend for creators including a picture, with a lowest rate of 39% in 2010 and a highest rate of 49% in 2016.

[25] I require matches have the same creator ethnicity, gender, and project category. I only include first-time creators without prior success and projects that were not Staff Picked. Among candidates matching all criteria, I select pairs with the most similar funding goals.

Background on Backers

I start this section with several important features of backers on Kickstarter and how they relate to project success. Table 29 provides complete tabulations.

Compared to creators, Kickstarter's website reveals significantly less information on backers. For all projects, the count of backers and total pledge amounts are observed, allowing the additional calculation of average pledge per backer. Unfortunately, I cannot make further inference about the distribution of backer support in terms of dollar amounts pledged. Once a project reaches ten or more backers, Kickstarter further reveals the city location(s) of backers, with up to ten locations and the number of backers per location provided. These data are scraped in the data collection phase.

With this baseline data, I first observe that project success on Kickstarter typically requires achieving support of at least 20 backers. There is a 5% success rate for projects with 10 or fewer backers, 58% success rate for projects with 11-20 backers, and 86% success rate for projects with 21+ backers. By contrast, the average pledge amount does not vary much over these quantities of backers. Thus, while projects in total average 78.6 backers as some exceed 1000+ backers, the success deterioration for minorities during difficult times is more due to projects that would have received, for example, 25 backers instead only getting 15.

I can next measure the spatial distance of creators to backers using the geographic centroids of cities and the Haversine flat earth formula. I call a "local backer" one that is in a city less than 50 miles from the creator's city, including being in the same city. Most backers are not local, with 36.1% of observed backers on average being within a 50-mile zone for a project. This local backer share is smaller at 20% if weighting projects by their number of backers.[26] Across categories, Food, Music, and Comics are the least localized, while Photography and Journalism are the most localized. This need to appeal to non-local backers is true at small funding amounts. The local share of observed backers for projects with 10-15 backers is 28%. By even this small level of support, 14% of backers are typically located in one the top 20 cities on Kickstarter in terms of backing (excluding the creator's own city if she is located in a top 20 city).

Finally, most backers on Kickstarter are likely to be white. Kickstarter does not publish individual backer names no longer. Therefore, I have this backer information for projects only from April 2009 to March 2012. I apply my ethnic names algorithms to the backer data, finding minorities account for 6.2% of backers from 2009-2012, with Black, Hispanic, and Asian shares of backers being 0.4%, 2.9%, and 2.7%, respectively.[27] The average minority backer share for a minority creator is 11% compared to 4% for a white creator during this initial period. If I further condition on basic project and creator traits, I estimate that the share of backers who are minorities is about 10% higher for projects developed by minority creators.

In summary, project success for creators typically hinges on building a critical mass of 20 or more backers, the majority of whom will tend to be white and from outside of the creator's local area. I next turn to split sample estimations keeping these features in mind.

<u>Breadth of Backers: Minority Backers</u>

The impact of challenging times for the level of minority backer support for minority

[26] These shares using the top 10 backer locations for a project are likely upper bounds on the total share of backers who are local. Sorting cities by their count of backers for a project, the top ranked city is local in 60.1% of cases, the second ranked city is local in 31.2% of cases, and the shares decline monotonically to 7.5% for the tenth ranked city. Thus, unobserved backers for a project in cities outside of the top 10 are more likely to be non-local.

[27] On the creator side, the Gafni et al. (2021) data are 1.3% Black, 2.7% Hispanic, and 2.2% Asian. The Hispanic and Asian shares for this earlier period are lower than my full sample due to the growth of minority creators over time as a share of Kickstarter, shown in Figure 4.

creators is theoretically ambiguous. Support might increase if communities rally around minority creators in difficult moments, such as the "activist homophily" documented by Greenberg and Mollick (2017) for gender-based crowd funding. However, anxious conditions might lead potential minority backers to be cautious and limited in the financial support they provide to causes. My first sample split explores if funding declines are more prominent among types of projects that typically depend heavily on minority support. While this group cannot explain all of the shortfall observed, it provides insight into whether declines in success likely embody retractions of support from potential backers close to the creator.

While I lack data on project backers after 2012, I use the initial period to segment the full sample by the degree to which it is likely that minority backers are important. I first separately calculate the average minority backer share by product category and by state with the Gafni et al. (2021) data for projects with a minority creator. These metrics describe the products and states where minority creators were more supported by minority backers. I then interact these two metrics for a product x state anticipated dependency. By using this interaction approach, I calculate anticipated dependency for the full sample even if a given {product, state} pair was not observed in the initial period. I then order the anticipated dependencies and create groups from lowest to highest anticipated dependency.

Columns (2)-(4) of Table 23 report results with this sample split. The strongest declines in minority success when fear spikes are found among the part of the distribution where minority backing was least likely, as seen in Column (2). Columns (3) and (4) also show negative coefficients, with the latter being precisely measured, but the estimated impact is less than half the size. The table reports the linear difference of the interaction terms of

Columns (2) and (4), which is sizable but I do not reject the results are the same. The relatively small share of backers who are minorities made it unlikely that the large success differentials for minority creators could be explained by reduced minority backer support. This analysis confirms this intuition and provides further evidence that minority backers may soften funding shortfalls for minority creators.

Breadth of Backers: Localized Projects and Project Size

Projects on Kickstarter range from local to global in appeal. For instance, a project to revive a local dance studio in Boston might only be funded by local residents and former customers, with little appeal to backers in Kansas City. By contrast, a project that proposes an audio book version of a popular comic book might garner national interest. Local projects could factor into funding declines if minority creators are more likely to draw localized support (including white backers) and, perhaps, these types of projects become less desirable to potential backers when fear spikes.

I test these features by splitting the sample based upon the degree to which backers for a product tend to be localized. Kickstarter's categories are mostly orthogonal to the local-global dimension, as described above, and so I develop a project-level classification. For projects with 10+ backers, I can calculate directly a "local" project as one that has 50% or more of its observed backers within 50 miles of the creator's city. For projects with fewer than 10 backers, I predict this likelihood by training a machine learning algorithm on the project blurbs (e.g., the blurb from the example in Figure 3 is "Woke Up Running. My first album of my thoughts and sometimes harsh reality of life. https://m.facebook.com/jgdreams"). The total estimated local share is 11.6%, comprised of a 10.6% rate for projects where I directly observe location and 12.8% rate for the predicted example.

The large sample of non-local projects in Column (5) of Table 23 is very similar to my main results. While they are not precisely estimated, I observe smaller coefficients in Column (6) among the local projects. Declines in local backer support do not appear a significant driver of the crowd-funding gaps for minorities during high levels of fear.[28]

Combining the limited influence of minority and local backers, it appears unlikely that a deterioration in “friends and family” or community support is responsible for the outcomes, although there are dimensions of affinity I cannot observe. Columns (2)-(4) of Table 24 provide one further split to consider whether the declines are isolated among projects with small funding goals, where affinity is most likely to be influential. I divide the sample at project goals of $2500 and $10,000. Projects with small targets show larger declines in funding success, but the results are also quite strong among projects with goals of $10,000 or more. These results again speak to a widespread effect.

Sample Splits by Political Leaning of Counties

Recent studies identify larger negative effects from polarizing political events in conservative areas, such as racial profiling in police traffic stops in counties after a Trump political rally (Grosjean et al. 2022) or acts of Asian Hate in Trump-leaning counties after Trump’s tweets of the “Chinese Virus” (Cao et al. 2022). Engelberg et al. (2022) measure partisan entrepreneurship in locations with political swings.

The high dependency of Kickstarter projects on non-local backers suggests the political leaning of creator’s city may matter less in the crowd funding context. Columns (5)-(7) of Table 24 split the sample into groups based upon average Republican vote shares in

[28] Descriptively, the sharpest funding deteriorations for minorities are in Comics, Games, Music and Publishing, which are among the most global categories.

Presidential elections since 2012, with divisions at 50% and 30%. Comparable to other studies, I find the largest deteriorations in conservative counties where the average Republican vote share exceeds 50%. Yet, I also find quite strong declines in liberal counties, too, where the Republican vote share is less than 30%. The linear difference between these extremes is not statistically significant. This speaks a rather balanced retraction in support during periods when the Migration Fear Index is high.

Tests of Backer Distributional Equality

Taking stock of Tables 23 and 24, I observe a widespread retraction of support that does not rely on minority or localized backers and is rather similar across funding goals and the political leaning of the creator's city. Descriptively, I have also observed that most projects that obtain robust support tend to garner some backers in the larger cities.

Table 25 analyzes whether the spatial distribution of backers across cities is different in times of low and high levels of the Migration Fear Index. While I have established the backer count is declining for minority creators, this test explores whether the retraction is "general" in nature. I specifically conduct equality tests of the city distribution of supporters of projects with a split of projects at a Migration Fear Index value of 1.75. Failing to reject the null hypothesis that the spatial distributions are the same leans against a hypothesis of acute decline of support for minorities in a few locations.

Panel A of Table 25 shows limited spatial differences in project backer support for projects with 10-20 backers, which is the critical range where funding success typically emerges. Column (1) considers the top 18 cities that each contain 1% or more of backers. Among projects with 10-20 backers, 21.1% of backers are in these 18 cities, with the share for minority creators a bit higher at 24%. More important, there are very small differences between quarters with low and high index values for white and minority creators. For both sets, I provide p-values for whether their relative backers are more or less present in the top 18 cities compared to elsewhere (share equality test) and whether the city distribution among the 18 cities is similar (distribution equality test). The latter employs the distribution comparison of Kaplan (2019). None of the four tests rejects the null hypothesis of spatial equality.

Column (2) lowers the city inclusion threshold to the 96 cities with a 0.1% share or greater. This is my preferred sample, and I continue to find spatial equality for backers. Figure 2 provides a graphical depiction. Columns (3) and (4) further lower the inclusion threshold, ultimately covering in Column (4) the 597 cities with at least 0.01% of backers. I am cautious regarding ever larger city spans due to two challenges in the distribution equality test. The test identifies deviations at points along the distribution and calculates a simulated p-value for overall distributional equality. The long tail is populated with cities that have just one or two instances of backers, and thus the null hypothesis will always be rejected when testing that the distributions are the same across all cities due to this lumpiness. The theoretical basis for the procedure is also uncertain in the presence of many ties, which one repeatedly encounters among smaller cities. Nonetheless, the test results remain comparable.

Panel B shows comparable distributions with backers from all projects. These distributions are dominated by projects that garner many backers, increasingly skewing them towards the biggest cities. The results are mostly similar, with most tests continuing to fail to reject a null hypothesis of spatial equality. Two differences are present. The first is that there is less backer support among the largest cities for minority creators that diminishes as the city span widens. For white creators, I also find some parts of the city distribution are different in

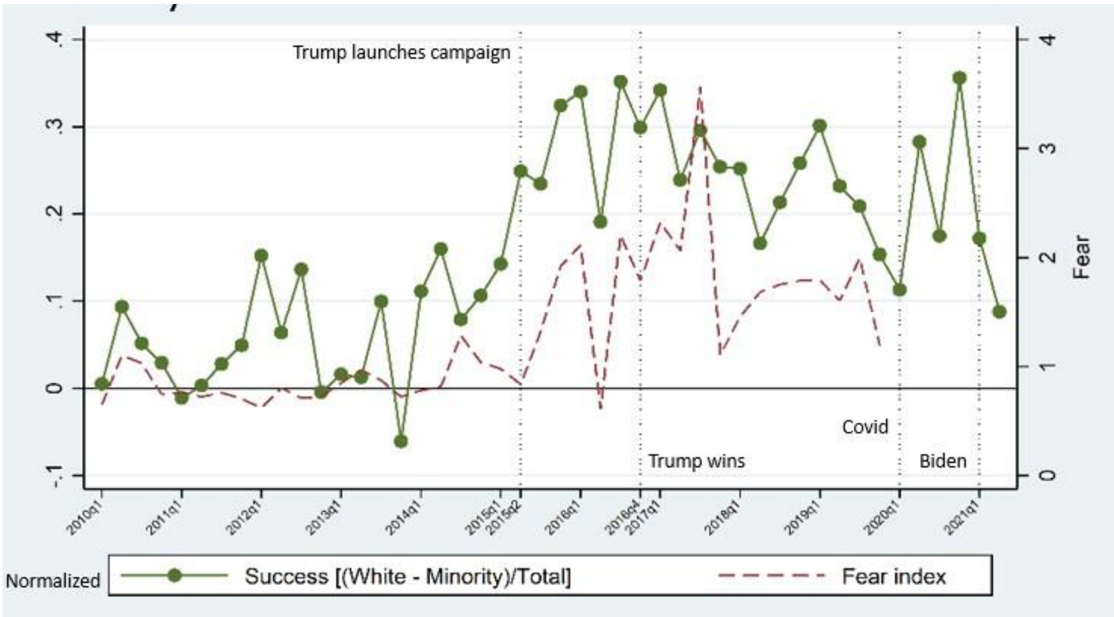
Columns (3) and (4) when the city distribution reaches 166 cities or more.

Conclusion

Financial capital is essential for many projects to be launched, and one of the hopes for crowd funding is that it will democratize access to capital from those previously excluded. Prior work has shown that discrimination still exists on crowd-funding sites, on both ethnic and gender lines, and I take a step further in understanding how minority creators can suffer funding shortfalls in moments when anxiety over immigration is high. I find large effects, such that the funding shortfalls faced by a minority creator doubles or triples when immigration fears are high.

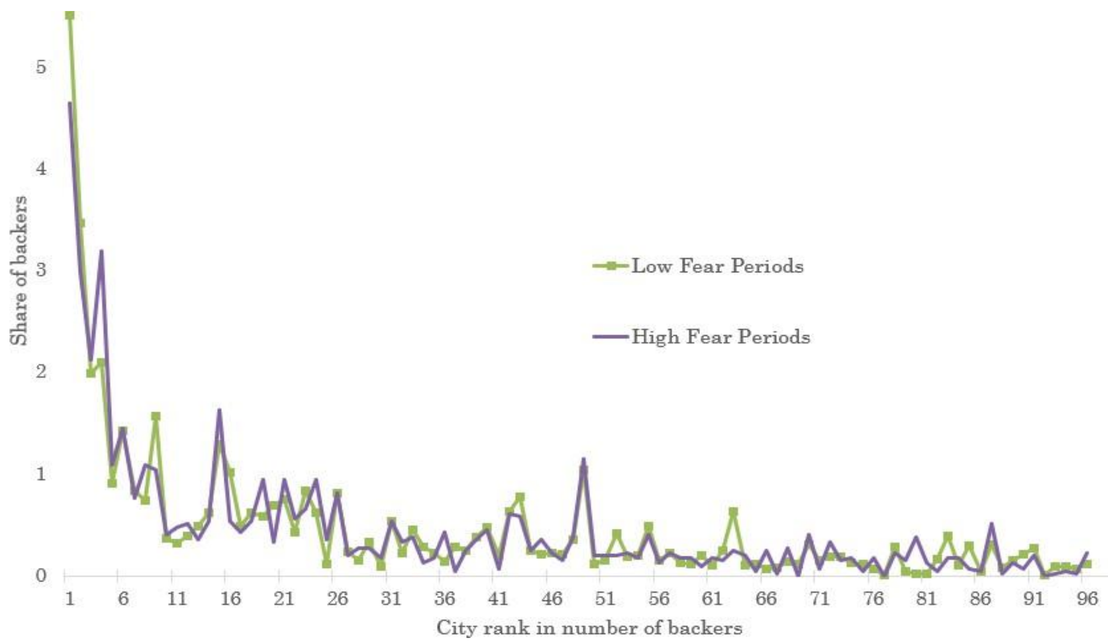
I examined three hypotheses for why minority creators might have lower support for their projects during periods of heightened migration fear. The analyses suggest that the link is not due to mechanical features like minorities specializing in certain types of projects. The evidence also casts doubt that the funding shortfalls were due to weakened support among potential backers with affinity for creators. Funding shortfalls are a bit worse for minorities in conservative parts of the country, but they are also quite strong in liberal areas. Recognizing potential shortfalls will often come from reduced support by non-local white backers, my final analysis fails to reject that the city distribution of backers is the same across levels of the Migration Fear Index. These results suggest a mostly uniform retraction of support occurs.

Figure 3. Success difference of white vs. minority created projects



Notes: The solid green line with circle markers shows the success difference of white- vs. minority-created projects on Kickstarter scaled by the total rate of project success: (success rate for white creators – success rate for minority creators)/(total average success rate). Values for this series are shown on the left-hand axis, with positive values indicating white creators are achieving their funding goals at a higher rate than minorities. The red dashed line is the Migration Fear Index first developed by Baker et al. (2015, 2016), divided by 100 and with values shown on the right-hand axis. The horizontal axis documents quarters.

Figure 4. City distribution of backers for minority projects with 10-20 backers



Notes: Figure shows the share of backers for minority projects across cities for projects with 10-20 backers. Cities are ranked by their total share of backers, with the 96 cities that account for at least 0.1% of backers included. The purple line with no marker shows the distribution in quarters with a Migration Fear Index value of 1.75 or higher; the green line with marker shows the distribution in quarters when the index is less than 1.75. Table 25 reports equality tests.

Table 15 Descriptive statistics of Kickstarter sample

This table reports the descriptive statistics on the 2009-2021 regression sample.

	Mean	SD	p25	Median	p75
	(1)	(2)	(3)	(4)	(5)
Crowding funding outcome					
Success	0.488	0.500	0	0	1
Pledges/Goal (cap of 1.25)	0.602	0.548	0.010	0.546	1.153
Backers	78.6	194.5	3.0	19.0	69.0
ln(Backers)	2.860	1.828	1.386	2.996	4.248
Creator characteristics					
Minority	0.093	0.290			
Black	0.011	0.104			
Hispanic	0.051	0.219			
Asian	0.031	0.174			
Pr(Minority)	0.142	0.243	0.014	0.048	0.117
Female	0.255	0.436	0	0	1
Immigration fear					
Fear	1.254	0.601	0.804	1.040	1.685
Google SVI	0.548	0.296	0.333	0.556	0.778
Project characteristics					
ln(Goal)	8.432	1.551	7.439	8.517	9.393
ln(Horizon)	3.479	0.359	3.434	3.434	3.584
ln(Total Projects)	0.033	0.147	0.000	0.000	0.000
ln(Length)	4.665	0.347	4.595	4.812	4.883
Self Mention	0.076	0.265			
Staff Picked	0.103	0.304			

Table 16 Migration fear and minority funding outcomes

This table reports coefficient estimates of linear probability regression (Model 1) and ordinary least squares regressions. I regress three funding outcome variables on the interaction of Minority and Fear. Success is an indicator equal to one if the project is successfully funded. Pledges/Goal is equal to total funding pledges scaled by project goals, capped at 125%. ln(Backers) is equal to the natural logarithm of the number of project backers. Minority is an indicator variable equal to one if a project creator is a minority. Fear is the Migration Fear Index created by Baker, Bloom, and Davis (2015, 2016) divided by 100. Project Controls include an indicator for female creator, log project target funding, log project description length, log project duration, the number of projects created by the same creator in the same year-quarter, and an indicator variable for whether the creator is self-mentioned in project description. Table 26 reports the coefficients for controls. Specifications include year, state and category fixed effects. Regressions are weighted so that each creator-quarter receives equal weight. Standard errors are two-way clustered by creator and quarter, with t-statistics reported in parentheses. *, **, and *** indicate significance at the 10, 5, and 1% levels, respectively.

	Success	Pledges/Goal	ln(Backers)
	(1)	(2)	(3)
Minority × Fear	**-0.032*** **	**-0.038*** **	**-0.147*** **
	(-4.90)	**(-4.69)**	**(-4.40)**
Minority	-0.021*	-0.025*	-0.104**
	(-1.96)	(-1.99)	(-2.11)
Fear	-0.010	-0.012	-0.042
	(-0.74)	(-0.79)	(-0.75)
Observations	150282	150282	150282
Adj. R^2	0.218	0.243	0.193
Project Controls	Y	Y	Y
Year, State and Category FE	Y	Y	Y
Mean of Outcome Var.	0.488	0.602	2.860
Impact of 1 SD of Fear	-0.019	-0.023	-0.088
Impact Relative to Mean	-3.94%	-3.79%	-3.09%

Table 17 Funding outcomes by minority ethnic group

See Table 16.

	Success	Pledges/Goal	ln(Backers)
	(1)	(2)	(3)
Black × Fear	**-0.023**	**-0.037****	**-0.095**
	(-1.26)	**(-2.12)**	**(-1.49)**
Black	-0.114***	-0.119***	-0.518***
	(-4.85)	(-4.79)	(-5.29)
Hispanic × Fear	**-0.040*****	**-0.046*****	**-0.174*****
	(-5.30)	**(-4.88)**	**(-4.25)**
Hispanic	-0.045***	-0.057***	-0.234***
	(-3.34)	(-3.60)	(-3.62)
Asian × Fear	**-0.025***	**-0.028***	**-0.132****
	(-1.98)	**(-1.95)**	**(-2.33)**
Asian	0.053***	0.063***	0.265***
	(3.04)	(3.15)	(3.32)
Fear	-0.010	-0.012	-0.042
	(-0.74)	(-0.79)	(-0.75)
Observations	150282	150282	150282
Adj. R^2	0.219	0.245	0.195
Project Controls	Y	Y	Y
Year, State and Category FE	Y	Y	Y
Black:			
Mean of Outcome Var.	0.290	0.369	2.113
Impact of 1 SD of Fear	-0.014	-0.022	-0.057
Impact Relative to Mean	-4.77%	-6.03%	-2.70%
Hispanic:			
Mean of Outcome Var.	0.360	0.451	2.331
Impact of 1 SD of Fear	-0.024	-0.028	-0.105
Impact Relative to Mean	-6.68%	-6.13%	-4.49%
Asian:			
Mean of Outcome Var.	0.512	0.634	3.057
Impact of 1 SD of Fear	-0.015	-0.017	-0.079
Impact Relative to Mean	-2.93%	-2.65%	-2.60%

Table 18 Non-linear specifications

See Table 16. Regressions remove the linear interaction of Minority x Fear and instead introduce two indicator variables for Minority x Fear High and Minority x Fear Medium, with Fear High and Fear Medium defined to be a Minority Fear Index value between [1.75, max] and [0.8, 1.75), respectively. These chosen points of 0.8 and 1.75 correspond to approximately the 25th and 75th percentiles, respectively, of the index from Table 15.

	Success	Pledges/Goal	ln(Backers)
	(1)	(2)	(3)
Minority × High Fear	**-0.058*** **	**-0.072*** **	**-0.294*** **
	(-3.85)	**(-4.45)**	**(-6.25)**
Minority × Medium Fear	**-0.044** **	**-0.051** **	**-0.180*** **
	(-2.38)	**(-2.48)**	**(-2.81)**
Minority	-0.024*	-0.028*	-0.123***
	(-1.82)	(-1.91)	(-2.87)
Fear	-0.011	-0.013	-0.046
	(-0.84)	(-0.86)	(-0.81)
Observations	150282	150282	150282
Adj. R^2	0.218	0.243	0.193
Project Controls	Y	Y	Y
Year, State and Category FE	Y	Y	Y

Table 19 Specification checks on Table 16

See Table 16. Each panel reports focal interaction term(s) from separate regressions.

Specification Check	Interaction Term(s)	Success
A: Baseline analysis	Minority x Fear	-0.032*** (-4.90)
B: Including Economic Policy Uncertainty Index	Minority x Fear	-0.033*** (-4.86)
	Minority x EPU-Index	-0.013 (-0.14)
C: Including Economic Policy Uncertainty Index (News)	Minority x Fear	-0.034*** (-4.95)
	Minority x EPU-News	-0.010 (-0.51)
D: Including Twitter-Based Uncertainty Index	Minority x Fear	-0.033*** (-4.79)
	Minority x Uncert-Twitter	-0.006 (-0.45)
E: Using pre-post on election of President Trump (sample period of Q4 2015 – Q4 2017)	Minority x Post	-0.045*** (-3.98)
F: Using alternative Google Search Value Index developed by authors	Minority x Google SVI	-0.060*** (-3.36)
G: Including Migration Fear Index for United Kingdom	Minority x Fear	-0.023*** (-2.98)
	Minority x Fear-UK	-0.007 (-1.51)
H: Including Migration Fear Index for Germany	Minority x Fear	-0.027*** (-4.23)
	Minority x Fear-DEU	-0.001 (-1.00)
I: Including Migration Fear Index for France	Minority x Fear	-0.032*** (-4.46)
	Minority x Fear-FRA	-0.001 (-0.12)
J: Including Bartik-style control for expected racial success	Minority x Fear	-0.032*** (-4.90)

Table 20 Specification checks on Table 16

See Table 16. Each panel reports focal interaction term(s) from separate regressions.

Specification Check	Interaction Term(s)	Success	
A: Baseline analysis	Minority x Fear	-0.032***	(-4.90)
B: Excluding all project controls	Minority x Fear	-0.038***	(-5.03)
C: Interacting all project controls with Fear index	Minority x Fear	-0.032***	(-4.78)
D: Excluding sample weights	Minority x Fear	-0.033***	(-4.59)
E: Weighing each creator equally	Minority x Fear	-0.025***	(-3.97)
F: Including City x Year Fixed Effects	Minority x Fear	-0.026***	(-2.90)
G: Including Category x Year Fixed Effects	Minority x Fear	-0.020***	(-3.31)
H: Excluding Staff Picked projects	Minority x Fear	-0.030***	(-4.84)
I: Dropping Kickstarter spike period of Q2 2014 – Q3 2015	Minority x Fear	-0.035***	(-4.62)
J: Using alternative name algorithm 1 to classify minority	Minority x Fear	-0.064***	(-4.74)
K: Using alternative name algorithm 2 to classify minority	Minority x Fear	-0.032***	(-4.90)
L: Using picture to classify minority status [n=72,854]	Minority x Fear	-0.029***	(-3.71)

Table 21 Specifications using matched sample from 2015-2017

See Table 16. Estimations include Project Controls and Year, State and Category Fixed Effects.

	Success	Pledges/Goal	ln(Backers)
	(1)	(2)	(3)
A. Minority creator matched sample			
Fear	-0.027***	-0.039***	-0.148***
	(-3.51)	(-3.86)	(-3.87)
Observations	2317	2317	2317
Adj. R^2	0.146	0.167	0.084
Mean of Outcome Var.	0.216	0.283	1.776
Impact of 1 SD of Fear	-0.016	-0.023	-0.089
Impact Relative to Mean	-7.51%	-8.28%	-5.01%
B. White creator matched sample			
Fear	0.009	0.007	0.013
	(1.14)	(0.71)	(0.38)
Observations	20672	20672	20672
Adj. R^2	0.177	0.204	0.112
Mean of Outcome Variable	0.302	0.387	2.122
Impact of 1 SD of Fear	0.005	0.004	0.008
Impact Relative to Mean	1.79%	1.09%	0.37%

Table 22 Specification using sample of creators with multiple projects

See Table 16. Estimations include Project Controls and Year, State and Category Fixed Effects. Estimations additionally add Creator Fixed Effects.

	Success	Pledges/Goal	ln(Backers)
	(1)	(2)	(3)
Minority × Fear	**-0.028****	**-0.027***	**-0.112*****
	(-2.16)	**(-1.96)**	**(-3.14)**
Fear	-0.003	0.000	0.012
	(-0.64)	(0.01)	(0.87)
Observations	40729	40729	40729
Adj. R^2	0.613	0.707	0.827
Mean of Outcome Variable	0.632	0.789	3.452
Impact of 1 SD of Fear	-0.017	-0.016	-0.067
Impact Relative to Mean	-2.66%	-2.06%	-1.95%

Table 23 Specifications with split sample by likelihood of minority or local backer support

See Table 16. Estimations include Project Controls and Year, State and Category Fixed Effects. Columns (2)-(4) split the sample based upon the likelihood that significant minority backing exists for a minority creator using data from Gafni et al. (2021) to make {State, Product} predictions. Columns (5) and (6) split the sample by whether 50% or more of backers are within 50 miles of the creator or likely to be so.

	Success in full sample estimations	Success for projects split into samples by likelihood of significant minority backing			Success for projects split into samples by local backing	
		Least minority	Moderate	Most minority	Majority distant	Majority local
	(1)	(2)	(3)	(4)	(5)	(6)
Minority × High Fear	**-0.032*****	**-0.056****	**-0.012**	**-0.022*****	**-0.034*****	**-0.024**
	(-4.90)	**(-2.36)**	**(-1.32)**	**(-3.21)**	**(-5.05)**	**(-1.33)**
Minority	-0.021*	-0.020	-0.052***	-0.021*	-0.022*	-0.009
	(-1.96)	(-0.57)	(-3.52)	(-1.82)	(-1.89)	(-0.39)
Fear	-0.010	-0.018	-0.001	-0.015	-0.008	-0.021
	(-0.74)	(-1.30)	(-0.08)	(-0.97)	(-0.61)	(-1.49)
Observations	150282	31420	50047	59098	132582	17473
Adj. R^2	0.218	0.199	0.196	0.258	0.219	0.235
			Linear Diff	-0.034	Linear Diff	-0.011
			Col. (2)-(4)	(-1.46)	Col. (5)-(6)	(-0.62)
Mean of Outcome Var.	0.488	0.501	0.487	0.498	0.498	0.416
Impact of 1 SD of Fear	-0.019	-0.034	-0.007	-0.013	-0.020	-0.014
Impact Relative to Mean	-3.94%	-6.72%	-1.48%	-2.66%	-4.10%	-3.47%

Table 24 Specifications with split sample by funding goal and local political climate

See Table 16. Estimations include Project Controls and Year, State and Category Fixed Effects. Columns (2)-(4) split the sample by funding goal of project. Columns (5)-(7) split sample based upon average Republican vote shares in Presidential elections since 2012.

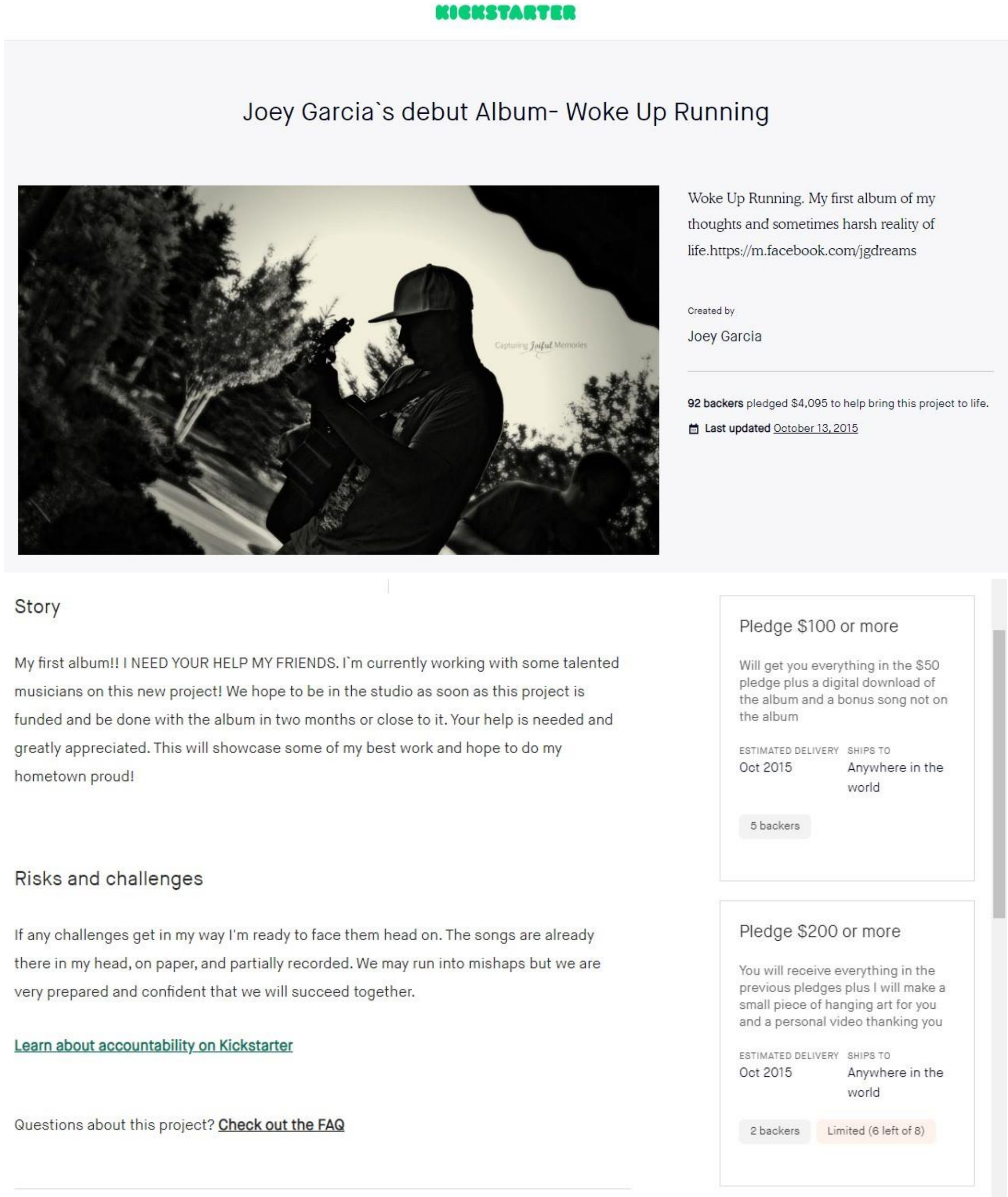
	Success in full sample estimations	Success for projects split into samples by funding goal			Success for projects split into samples by Republican vote share of creator county		
		($0, $2500]	($2500, $10,000]	($10,000, max)	(50%, 100%)	(30%, 50%]	(0%, 30%]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Minority × High Fear	**-0.032*** **	**-0.039*** **	**-0.037*** **	**-0.025** **	**-0.058*** **	**-0.017* **	**-0.032*** **
	(-4.90)	**(-3.51)**	**(-3.52)**	**(-2.08)**	**(-4.12)**	**(-1.81)**	**(-3.21)**
Minority	-0.021*	-0.016	-0.023	-0.012	0.003	-0.053***	-0.022
	(-1.96)	(-0.82)	(-1.51)	(-0.64)	(0.11)	(-2.95)	(-1.49)
Fear	-0.010	-0.010	-0.014	-0.004	-0.010	-0.012	-0.014
	(-0.74)	(-0.56)	(-0.99)	(-0.50)	(-0.72)	(-0.84)	(-1.14)
Observations	150282	52253	58607	39422	25804	54191	52016
Adj. R^2	0.218	0.147	0.180	0.244	0.226	0.217	0.193
			Linear Diff	-0.015		Linear Diff	-0.024
			Col. (2)-(4)	(-1.09)		Col. (5)-(7)	(-1.34)
Mean of Outcome Var.	0.488	0.611	0.497	0.311	0.390	0.444	0.586
Impact of 1 SD of Fear	-0.019	-0.023	-0.022	-0.015	-0.035	-0.010	-0.019
Impact Relative to Mean	-3.94%	-3.84%	-4.47%	-4.83%	-8.94%	-2.30%	-3.28%

Table 25 Distribution analysis for backers

This table reports measures of backer distribution among projects with 10 or more backers. Reported p-values test for equality of backer distribution across low and high levels of the Migration Fear Index, dividing the sample at index value of 1.75 and using Kaplan (2019) simulated p-value function.

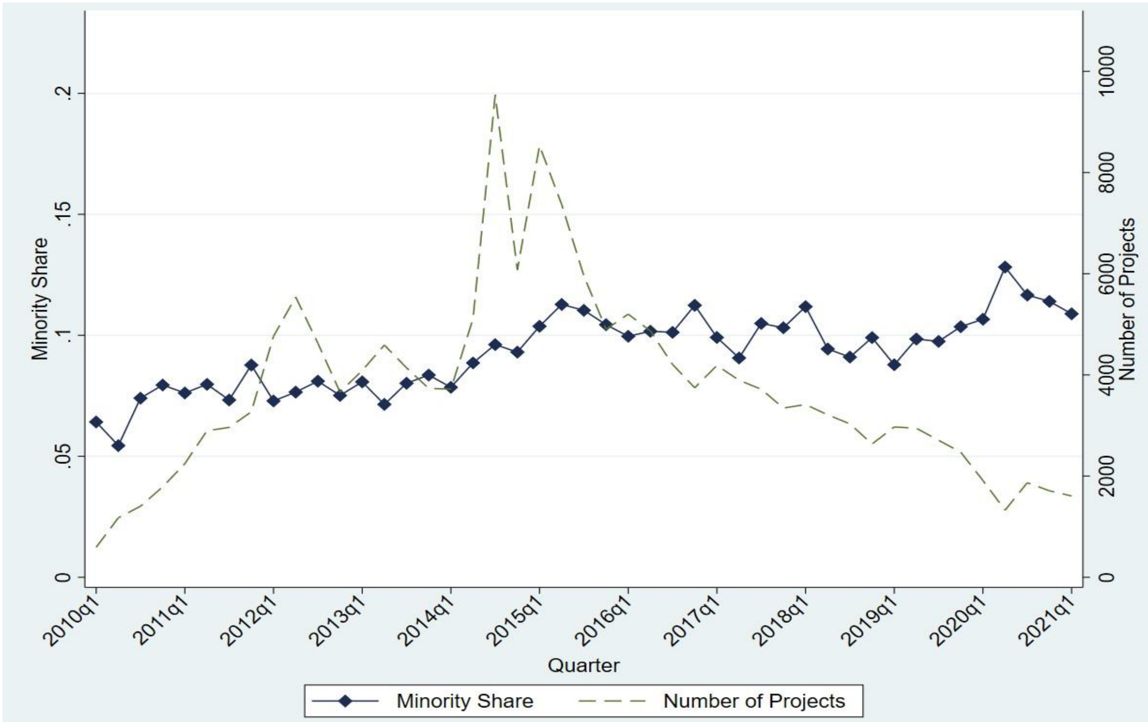
	City distribution restricted to 18 cities with >1% total backer share	City distribution restricted to 96 cities with >0.1% total backer share	City distribution restricted to 166 cities with >0.05% total backer share	City distribution restricted to 597 cities with >0.01% total backer share
	(1)	(2)	(3)	(4)
A. Projects with 10-20 backers				
Cumulative share of all backers	0.211	0.418	0.496	0.690
White creators low fear	0.210	0.419	0.496	0.689
White creators high fear	0.205	0.405	0.485	0.679
p-value: share equality test	0.813	0.453	0.565	0.592
p-value: distribution equality test	0.777	0.799	0.876	0.194
Minority creators low fear	0.242	0.452	0.532	0.730
Minority creators high fear	0.239	0.450	0.520	0.718
p-value for share difference	0.848	0.932	0.615	0.651
p-value: distribution equality test	0.995	0.995	0.354	1.000
B. All projects				
Cumulative share of all backers	0.572	0.768	0.818	0.907
White creators low fear	0.563	0.760	0.812	0.903
White creators high fear	0.584	0.780	0.827	0.911
p-value: share equality test	0.492	0.506	0.631	0.800
p-value: distribution equality test	0.500	0.311	0.009	0.001
Minority creators low fear	0.628	0.805	0.848	0.928
Minority creators high fear	0.532	0.746	0.797	0.901
p-value for share difference	0.007	0.136	0.200	0.499
p-value: distribution equality test	0.468	0.959	0.603	0.277

Figure 5. Example of Kickstarter Campaign



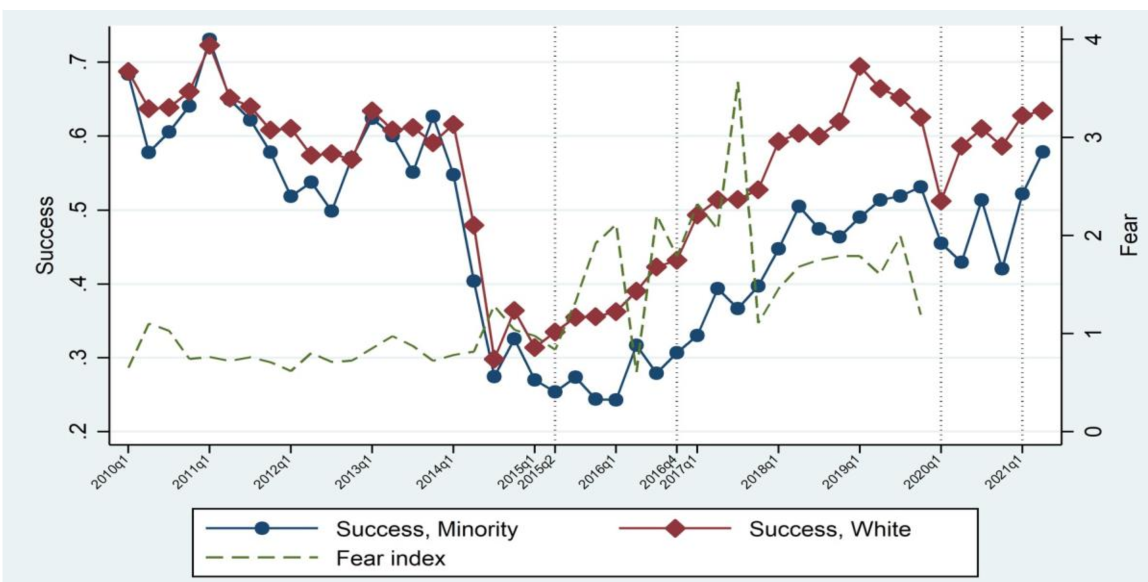
Source: https://www.kickstarter.com/projects/jgmusic/joey-garcia-s-debut-album-woke-up-running?ref=discovery&term=garcia (accessed March 2022).

Figure 6. Project count and minority share by quarter



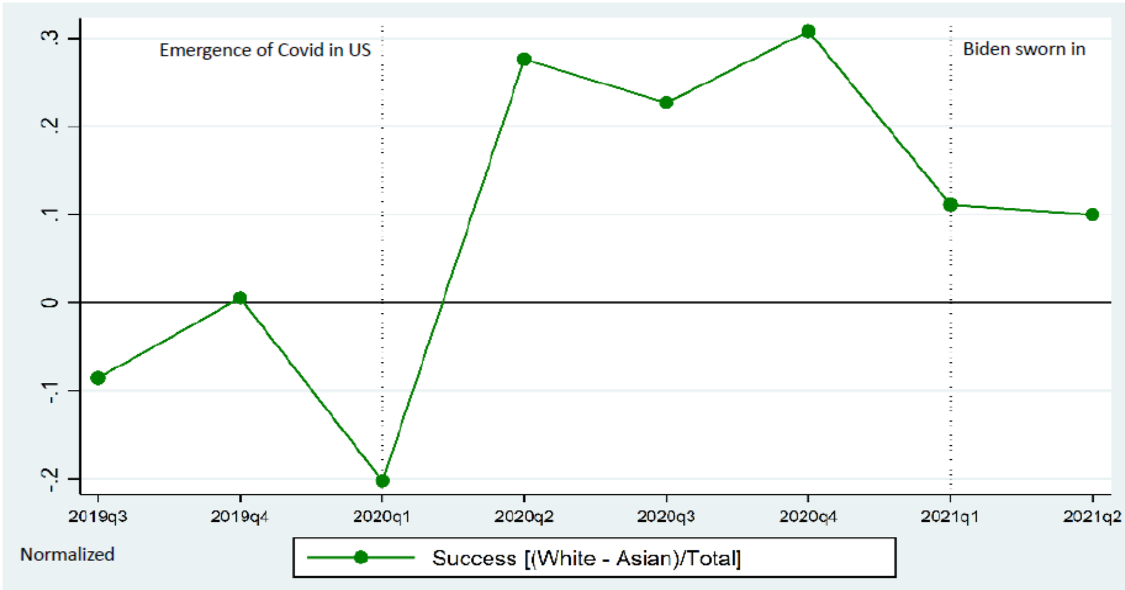
Notes: See Figure 3.

Figure 7. Raw success rates by minority status



Notes: See Figure 3.

Figure 8. Success difference of white vs. Asian creators during pandemic



Notes: See Figure 3.

Table 26 Full regression results for Table 16

See Table 16.

	Success	Pledges/Goal	ln(Backers)
	(1)	(2)	(3)
Minority × Fear	**-0.032*** **	**-0.038*** **	**-0.147*** **
	(-4.90)	**(-4.69)**	**(-4.40)**
Minority	-0.021*	-0.025*	-0.104**
	(-1.96)	(-1.99)	(-2.11)
Fear	-0.010	-0.012	-0.042
	(-0.74)	(-0.79)	(-0.75)
Female	0.072***	0.076***	0.264***
	(20.60)	(20.22)	(19.64)
ln(Goal)	-0.069***	-0.088***	0.122***
	(-19.63)	(-22.89)	(10.08)
ln(Horizon)	-0.135***	-0.150***	-0.376***
	(-16.96)	(-15.79)	(-11.22)
ln(Total Projects)	-0.184***	-0.173***	-0.544***
	(-9.76)	(-8.44)	(-7.77)
ln(Length)	0.026***	0.029***	0.140***
	(4.60)	(4.41)	(5.16)
Self-mention	0.122***	0.143***	0.524***
	(20.34)	(21.25)	(20.94)
Observations	150282	150282	150282
Adj. R^2	0.218	0.243	0.193
Project Controls	Y	Y	Y
Year, State and Category FE	Y	Y	Y

Table 27 Baseline with picture-based sample

See Table 16.

	Success	Pledges/Goal	ln(Backers)
	(1)	(2)	(3)
Minority × Fear	**-0.029*** **	**-0.034*** **	**-0.143*** **
	(-3.71)	**(-4.88)**	**(-6.55)**
Minority	-0.051***	-0.063***	-0.244***
	(-4.60)	(-5.78)	(-6.69)
Fear	-0.004	-0.006	-0.021
	(-0.27)	(-0.38)	(-0.40)
Observations	72854	72854	72854
Adj. R^2	0.221	0.245	0.197
Project Controls	Y	Y	Y
Year, State and Category FE	Y	Y	Y

Table 28 Decomposed baseline with picture-based sample

See Table 16.

	Success	Pledges/Goal	ln(Backers)
	(1)	(2)	(3)
Black × Fear	**-0.026****	**-0.035*****	**-0.192*****
	(-2.18)	**(-3.12)**	**(-4.99)**
Black	-0.184***	-0.216***	-0.750***
	(-10.76)	(-12.43)	(-12.43)
Hispanic × Fear	**-0.037*****	**-0.040*****	**-0.110*****
	(-3.14)	**(-3.32)**	**(-2.82)**
Hispanic	-0.026	-0.038**	-0.231***
	(-1.52)	(-2.15)	(-4.21)
Asian × Fear	**-0.022****	**-0.025*****	**-0.109*****
	(-2.49)	**(-3.28)**	**(-4.60)**
Asian	-0.010	-0.015	-0.068*
	(-0.83)	(-1.26)	(-1.85)
Fear	-0.005	-0.007	-0.025
	(-0.33)	(-0.45)	(-0.47)
Observations	72854	72854	72854
Adj. R^2	0.228	0.253	0.207
Project Controls	Y	Y	Y
Year, State and Category FE	Y	Y	Y

Table 29 Descriptive statistics of Kickstarter backers

This table reports descriptive statistics by levels of backers in projects. For projects with 10 or more backers, Kickstarter releases information on the top 10 backer locations and counts of backers in those 10 locations.

					Calculations using top 10 backer locations data		
Backer count	Project count	Project success rate	Minority creator share	Average pledge	Share of backers within 50 miles	Share of projects with more than 50% of backers with 50 miles	Share of backers in 18 largest Kickstarter backer cities (excl. own city)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
0-4	48008	0.015	0.115	42.1	n.a.	n.a.	n.a.
5-9	13180	0.156	0.093	67.2	n.a.	n.a.	n.a.
10-14	8448	0.378	0.088	79.9	0.280	0.230	0.134
15-19	6800	0.555	0.086	83.3	0.302	0.257	0.129
20-24	5699	0.644	0.085	84.8	0.326	0.298	0.132
25-29	5136	0.702	0.084	84.0	0.346	0.337	0.140
30-34	4598	0.760	0.082	81.9	0.366	0.375	0.144
35-39	3984	0.785	0.085	86.4	0.373	0.379	0.158
40-44	3638	0.826	0.079	85.1	0.394	0.413	0.162
45-49	3149	0.829	0.082	83.1	0.399	0.419	0.176
50-74	12388	0.873	0.077	84.9	0.425	0.460	0.192
75-99	7809	0.901	0.076	86.1	0.438	0.474	0.229
100-249	17146	0.939	0.076	81.4	0.411	0.421	0.309
250-499	5790	0.967	0.083	74.9	0.310	0.259	0.479
500-999	2606	0.985	0.074	67.1	0.208	0.116	0.631
1000*	1903	0.989	0.065	123.3	0.120	0.017	0.771
Total	150282	0.488	0.093	71.8	0.361	0.354	0.239

Table 30 Asian crowd-funding success during the pandemic

See Table 16. Regressions consider projects from 2019q1 to 2021q1, and I model an indicator variable After for the Covid period and interact it with indicator variables for creators being Chinese, South Asian (Indian), Other East Asian (Japanese, Korean, Vietnamese), Hispanic, and Black.

	Success	Pledges/Goal	ln(Backers)
	(1)	(2)	(3)
Chinese x After	**-0.179***** **(-5.07)**	**-0.163***** **(-3.75)**	**-0.250** **(-1.37)**
Chinese	0.086** (2.44)	0.078* (1.92)	0.223 (1.55)
South Asian x After	-0.073 (-1.10)	-0.040 (-0.54)	0.254 (0.93)
South Asian	-0.020 (-0.41)	-0.022 (-0.45)	-0.277** (-2.38)
Other East Asian x After	0.023 (0.39)	0.071 (1.25)	0.449** (2.34)
Other East Asian	-0.011 (-0.20)	-0.006 (-0.11)	0.005 (0.03)
Hispanic x After	0.049* (2.19)	0.046* (2.04)	0.207* (2.13)
Hispanic	-0.103*** (-4.71)	-0.113*** (-5.25)	-0.387*** (-4.18)
Black x After	-0.014 (-0.36)	-0.019 (-0.43)	-0.157 (-1.01)
Black	-0.110*** (-7.07)	-0.141*** (-8.53)	-0.422*** (-6.19)
After indicator	0.067*** (9.94)	0.086*** (14.88)	0.154*** (8.60)
Observations	16420	16420	16420
Adj. R^2	0.294	0.331	0.278
Project Controls	Y	Y	Y
Year, State and Category FE	Y	Y	Y

CHAPTER 4

MIGRATION FEAR AND FORECAST ACCURACY OF ETHNIC MINORITY ANALYSTS

Related Literature and Hypothesis Development

As indispensable intermediaries in the financial markets, sell-side analysts furnish pivotal information on anticipated firm performance, meeting the informational needs of their clientele (Bai et al., 2023). Clement et al. (2011) elucidate that analysts utilize stock returns and peers' revisions as a basis to forecast future earnings, a practice integral to shaping investor expectations and guiding investment decisions (e.g., Hodge, 2003; Hirshleifer et al., 2019). The literature extensively documents the biases and motivations that influence financial analyst outputs' accuracy (Ke and Yu, 2006; Mayew, 2008). Harford et al. (2019) note that analysts strategically direct more effort towards portfolio firms of greater career significance. Similarly, Rupar, Wang, and Yoon (2024) reveal the propensity of minority analysts to respond more negatively to unfavorable news from firms lacking in Diversity, Equity, and Inclusion (DEI) commitments, hinting at intrinsic subconscious biases. Moreover, Pursiainen (2022) correlates analysts' optimistic stock recommendations with a positive cultural trust bias stemming from their native country towards the firm's headquarter location. Bradshaw et al. (2019) document that analysts operating in jurisdictions with robust institutional frameworks tend to exhibit less optimism in target pricing and derive more value-relevant target prices.

The literature also provides ample evidence of the influence of the information environment on analysts' economic assessments. The importance of management access in earnings forecast accuracy is emphasized by Brown et al. (2015) and Green et al. (2014), whereas Flam et al. (2023) demonstrate the underrepresentation and reduced engagement of ethnic minority analysts in earnings conference calls.

In the context of this paper's focus, the influence of macro-level racial sentiment on minority analysts' forecast accuracy is scrutinized. The potential for analysts' economic evaluations to be hindered by personal moods or sentiments is supported by the findings of Wright and Bower (1992), Wann, Dolan, McGeorge, and Allison (1994), Forgas (1995), and Kang (2020), with Kang (2020) particularly illustrating the impact of significant socio-political events on the economic decision-making processes through the lens of minority CEOs' more pessimistic earnings forecasts post such events.

Hypothesis 1: The Fear Index negatively influences the information processing abilities of minority analysts due to psychological and external factors, thereby impairing their forecasting accuracy.

Delving into the backdrop of increased scrutiny faced by minority analysts, Law and Zuo (2021) observe a heightened likelihood of complaints against minority advisors during periods of intensified immigration fear. This observation suggests that external pressures, particularly those related to racial sentiment, might incentivize minority analysts to enhance their analytical rigor. Such an environment potentially necessitates greater effort from these analysts to maintain or improve the accuracy of their forecasts, serving as a mechanism to offset or navigate the adverse impacts of such societal dynamics.

Hypothesis 2: The Fear Index is associated with increased effort among minority analysts, leading to improved accuracy in their earnings forecasts and reflecting an adaptive response to elevated fear and anxiety levels.

Data, Sample, and Methodology

Identifying Minority Analysts

I analyzed data from January 1990 to December 2021, which includes U.S. sell-side analysts and the companies they assess in the combined CRSP/COMPUSTAT dataset. Using the I/B/E/S detailed history recommendation database and Thomson Reuters Investext, I acquired the complete names and brokerage affiliations of sell-side analysts who provided EPS forecasts. With the first name and the last name of the analysts, I use NamePrism API service to classify the ethnicity of the analyst into the one of following ethnicity groups which has the highest predicted probability: White, Black, Asian, and Hispanic and label all non-White analysts as minority analysts. Among all 3,628 analysts identified above, my minority analysts sample includes 462 analysts (12.73%), among which 384 analysts (10.58%) are Asian, 46 analysts (1.27%) are Hispanic, and 32 analysts (0.88%) are Black. Consistent with the sample composition of Kumar, Rantala, and Xu (2021), my data suggest Asians are the predominant minority group among financial analysts, followed by Hispanics and Blacks.[29]

Measure of Migration Fear

In this study, I utilize the Migration Fears Index developed by Baker, Bloom, and

[29] To identify analysts with an East-Asian ethnicity, I use Ethnicolr algorithm (https://github.com/appeler/ethnicolr) to infer the ethnicity of the sell-side analysts into one of the following: Black, Indian, Hispanic, Muslim, and East Asian.

Davis (2015, 2016) as my primary measure of migration uncertainty. This index provides a quantitative assessment of migration-related fears by analyzing the proportion of newspaper articles containing specific terms associated with migration and fear, relative to the total number of articles published in a given calendar quarter. The migration-related terms used in the index include "immigration, migration, assimilation, migrant, immigrant, asylum, refugee, open borders, border control, Schengen, and human trafficking." On the other hand, the fear-associated terms encompass "anxiety, panic, bomb, fear, crime, terror, worry, concern, and violent." By examining the presence of these terms in newspaper articles, the index offers insights into the extent to which migration concerns are being discussed and associated with fear during a particular period.

The Migration Fears Index has gained significant recognition and is widely employed in academic research due to its ability to accurately capture the quantitative intensity of migration-related fears. For example, Bloom, Davis, and Baker (2015) showed that the index's highest point in European nations occurred in 2015, aligning with the arrival of refugees from the Middle East and North Africa (Riaz, 2024). This validates the index's effectiveness in capturing the fluctuations and impact of migration-related fears.

Analyst Performance Measures and Control Variables

As I hypothesized in previous section, the time-varying social altitude towards immigrants captured by migration fear index and political environment would likely affect the information acquisition cost of minority sell-side analysts and subsequently affect the accuracy of their EPS forecasts. To objectively gauge the performance of sell-side analysts, I measure analyst performance with the absolute forecast error (AFE), which equals to the absolute difference between the analyst’s EPS forecast and the actual EPS for the firm-quarter, scaled by the closing stock price before the forecast date. (Clement, 1999; Clement & Tse, 2003; Merkley et al., 2020). In my further analysis, I use signed version of the absolute forecast error to identify the direction of the bias.

I include a large set of controls for analyst, firm, forecast, and brokerage house characteristics following prior literature on determinants of analysts forecast accuracy (Clement, 1999). The control variables include: a *Female Analyst* indicator to indicate the gender of the analyst, *General Experience* proxies for the analyst's working experience since the first forecast issued by the analyst, *Number of Firms* followed by the analyst in the quarter, *Size* and *Tobin* to capture the size and valuation of them firm, *Horizon* for the number of days between the forecast issue date and the earnings announcement date, *Number of Analysts* following the firm in the quarter, and *Brokerage Size* that equals to the number of analysts affiliated with the brokerage house in the quarter.

I obtain quarterly analyst forecasts and the firm-level control variables from COMPUSTAT and CRSP. The sample includes only the latest forecasts by the analyst for the target firm for the given forecasting period. My final merged regression sample consists of 897,708 quarterly EPS forecasts and 33,835 analyst-year observations for 6,341 unique firms. The appendix Table describes measures of all key variables and controls.

Descriptive Statistics

Table 31 presents the descriptive statistics for the variables used in the main regression analyses. Among the full sample of analyst quarterly forecasts included in the sample, minority analyst issued 10.1% of all quarterly EPS forecasts; the average forecast error is about 12.6% of lagged stock price. An average analyst follows 8.8 firms and is affiliated to brokerage houses that employ 17.4 analysts. The sample forecasts on average are issued 40.7 days ahead of the fiscal quarter end, and firms in the sample have an average of 7.5 analysts following them. Consistent with prior literature (Peng, Teoh, Wang and Yan, 2022), I find female analysts account for about 10% of all quarterly forecasts.

Results

In this section, I first examine the relationship between levels of migration fear in the United States and the forecast accuracy of minority analysts. To begin, I establish a baseline specification that allows me to assess the overall impact of migration fear on the forecasting accuracy of minority analysts. In my further analysis, I investigate whether this relationship is steered in one direction. To further strengthen and validate my findings from the main test, I introduce two significant environmental shocks that have had a profound influence on societal attitudes towards immigrants: the 2016 presidential election and the COVID-19 pandemic. These shocks have led to substantial fluctuations in migration fear and are relatively less correlated with changes in firm-level and analyst-level characteristics. Consequently, their inclusion helps address concerns regarding potential omitted correlated variables at the analyst or firm level, ultimately enhancing the robustness of my analysis. By considering these additional factors, I can more confidently evaluate the association between migration fear and the forecast accuracy of minority analysts, as they provide complementary evidence that aligns with my main findings. This approach allows me to strengthen the validity of my conclusions and mitigate potential alternative explanations for the observed effects.

Baseline Regression– Migration Fear and Forecast Accuracy of Minority Analysts

For the baseline estimation of the relationship between migration fear and forecast accuracy, I run OLS regression with the following model specifications:

$$AFE_{i,j,t} = \beta_0 + \beta_1 Minority_i \times Fear_t + \beta_2 Minority_i + \beta_3 Fear_t + \gamma Controls + \varepsilon_{i,j,t} \quad (1)$$

where *Minority* equals to one if the analyst's inferred ethnicity is Black, Hispanic, or Asian, and zero otherwise. *Fear* is the migration fear index by Baker, Bloom and Davis (2015). The *Controls* are firm, analyst, forecast, and brokerage house characteristics. I include a combination of analyst, firm and time fixed effects in the regressions to account for unobservable characteristics that vary across these dimensions. I report t-statistics with standard errors clustered at the analyst and quarter level.

Table 32 reports the regression results. In all six columns, the interaction term of *Minority* and *Fear* are positively correlated with *AFE* at 1% level, while the stand-alone *Minority* indicator variables are negatively and significantly correlated with *AFE* in column 1 and column 4. Taken together, these regression results suggest that although on-average minority analysts' forecast are more accurate than that of the non-minority analysts, minority analysts' forecast become less accurate when migration fear is high, as their forecast errors are positively correlated with migration fear. The magnitudes of coefficients are economically meaningful, a one-standard-deviation increase in the Migration Fear index (0.481) is associated with 1.91% increase in the absolute forecast error (i.e. decrease in forecast accuracy) over the mean AFE and is comparable to the 1.55% to 3.58% magnitudes for analysts' prior industry-related experience coefficient of accuracy as reported by Bradley, Gokkaya and Liu (2017). Interestingly, the standalone *Fear* variables are not statistically significant in any columns, suggesting that the negative impact of migration fear does not present on non-minority analysts and is unique to only minority analysts. In sum, the baseline results show that migration fear imposes significant negative impact on minority analysts' forecast accuracy.

Baseline Regression– Migration Fear and Forecast Bias of Minority Analysts

Building on the analytical framework outlined in previous section, this section delves into the directional bias of forecast inaccuracies by minority analysts. To facilitate this investigation, a signed measure of forecast accuracy, termed *Forecast Bias*, is employed. Consistent with existing scholarly work, a positive and larger value of this measure indicates an optimistic forecast, whereas negative and lesser values suggest analyst pessimism. Following the methodology outlined in equation (1), *Forecast Bias* is regressed against the interaction of *Minority* status and *Fear* index variables. Additionally, to mitigate the influence of unobservable heterogeneity across analysts, firms, and time, the regression model incorporates a comprehensive set of fixed effects for these dimensions. The statistical significance of the results is evaluated using t-statistics, with standard errors clustered by analyst and quarter, to ensure robustness.

Table 33 presents the outcomes of the ordinary least squares (OLS) regression analysis. The interaction term's coefficient is consistently negative and statistically significant at the 1% level across various models, suggesting a direct relationship between an elevation in the fear index and an increase in analyst forecast pessimism. Specifically, a one standard deviation increase (0.48) in the fear index is linked to an 18% increase in pessimism compared to the baseline average. The coefficient associated with the *Minority* status indicator does not significantly differ from zero, indicating the absence of an inherent bias towards pessimism among minority analysts. Echoing the findings from Table 32, the Fear index's coefficient itself is not statistically significant, underscoring that the observed effects are particularly relevant for minority analysts.

The 2016 Presidential Election and Forecast Accuracy of Minority Analysts

In this section, I leverage the 2016 U.S. presidential election as an exogenous shock to social migration fear and analyze its impact on the forecast accuracy of minority analysts. The unexpected victory of Donald Trump brought about a sudden change in the government's immigration policy. The president's campaign and subsequent stance on immigration contributed to the creation and intensification of immigration fears among certain segments of the population. Following the 2016 U.S. presidential election, the Migration Fear index experienced a notable increase in the United States. The index averaged 227 in 2017, compared to 168 in 2016 and 127 in 2015. This suggests a heightened level of migration fear in the country following the election, as reflected in media coverage and public discourse.

Previous research has demonstrated that migration fear can have adverse effects on individuals' willingness to communicate with minorities. Considering the role of analysts as intermediaries between firms and investors, social interactions play a crucial role in their ability to obtain and communicate private information about the companies they cover. The intense migration fear prevailing during this period can potentially increase the information acquisition costs for analysts by reducing the willingness of the opposite party to communicate with minority analysts. This can impede the flow of necessary information and hinder the accuracy of their forecasts.

By examining the impact of the 2016 U.S. presidential election on migration fear and its potential consequences for minority analysts, I gain insights into the broader implications of social attitudes and concerns on the forecasting accuracy within the financial industry.

I utilize the Donald Trump’s victory in the 2016 presidential election as a shock to migration fear and estimate the following OLS model in a Difference-in-Difference setting:

$$AFE_{i,j,t} = \beta_0 + \beta_1 Minority_i \times Trump_t + \beta_2 Minority_i + \beta_3 Trump_t + \gamma Controls + \varepsilon_{i,j,t} \qquad (2)$$

where *Trump* is an indicator variable equals to one if the analyst forecast is issued after the 2016 presidential election, and zero otherwise; *Minority* equals to one if the analyst's inferred ethnicity is Black, Hispanic, or Asian, and zero otherwise. The model includes the same set of control variables and fixed effects as the main regression model.

Table 34 reports the regression results. In all six columns β_1 are positive and significant at 1% level, suggesting that the forecast error differences between minority analysts and non-minority analysts become greater after Trump won the presidential election in 2016. Consistent with my hypothesis that intensified migration fear impair the ability to accurately process and integrate firm-relevant information into forecasts. The economical magnitude of the increase in absolute forecast errors associated with the presidential election ranges from 3.97% to 7.94% of the mean value of absolute forecast error.

In the supplementary analyses, which are not tabulated, I further investigate whether the inaccuracies in forecasts made by minority analysts exhibit a directional bias. Following a methodological approach similar to that described in equation (2), *Forecast Bias* is regressed on the interaction between *Minority* status indicator and the occurrence of the Trump election. These analyses yield consistent results, demonstrating that minority financial analysts are inclined to issue forecasts that are more pessimistic in nature subsequent to the election. Such findings lend robust support to the hypothesis that elevated levels of fear and anxiety adversely impact the capacity of minority analysts to accurately evaluate economic conditions, thereby confirming the causality of these effects.

In Table 35, I empirically examine the parallel trends in forecast accuracy spread between minority analysts and non-minority analysts during the 2016 presidential elections. I include time indicators variables and their interaction terms with *Minority* to capture the effect of the election at various time intervals. I find that the coefficients are not statistically significant in the t-2 year and t-1 year, refuting the possibility that the finding in Table 34 is caused by any time trend effect. In addition, the results suggest that the effect of presidential elections become significant in year 0 and is both statistically and economically strongest during year t+1, the effect persists during the three years after the 2016 presidential election.

To further alleviate my concern that the relationship I document above are caused by time-varying correlated omitted variables, and not *Minority* identification, in Table 36, I estimate a placebo test by randomly assign an analyst as minority and repeat the test in Table 34 over three sample periods. The coefficients of *Minority X Trump* are not significant in any of the nine-columns, further confirmed the robustness of the Diff-in-Diff results I find in Table 34.

This set of results suggest that the 2016 presidential election impairs the forecast accuracy of minority analysts over non-minority analysts. The negative impact on minority analysts by the presidential election was not present before the election and is long-lasting for up to three years after the event.

COVID-19 pandemic and Forecast Accuracy of Minority Analysts

The outbreak of COVID-19 in the United States in early 2020 resulted in an unfortunate increase in incidents of hate and discrimination against the East Asian population. Additionally, President Donald Trump's public statements on Twitter referring to COVID-19 as the "China virus" or "Kung flu" have further stigmatized Asians and contributed to an environment that fosters hate incidents against East Asians. I utilize Donald Trump's "China virus" tweet as an environmental shock to measure the impact of increased migration fear on analysts of East Asian origin. Through this approach, I test whether this exogenous shock in migration fear has a negative effect on the information acquisition and production by analysts of East Asian ethnicity, relative to analysts from other ethnic backgrounds.

To conduct this analysis, I employ an Ordinary Least Squares (OLS) regression model, which allows me to examine the relationship between migration fear and information-related outcomes for analysts. By comparing the experiences and performance of East Asian analysts with those of analysts from different ethnicities, I can gain insights into the potential adverse effects of heightened migration fear on information acquisition and production within this specific group.

I use the following regression model to capture the potential impact of migration fear on East Asian analysts and shed light on the challenges they face in obtaining and producing relevant information. This analysis contributes to my understanding of the broader implications of migration fear and its impact on the financial industry:

$$AFE_{i,j,t} = \beta_0 + B_1 Ethnicity_i \times COVID_t + B_2 Ethnicity + \beta_3 COVID_t + \gamma Controls + \varepsilon_{i,j,t} \tag{3}$$

where *Ethnicity* is a group of indicator variables that set to one for analysts with Black/East Asian/Indian/Hispanic/Muslim ethnicity predicted by Ethnicolr algorithm.

Table 37 reports the regression results. In all six columns the interaction term of East Asian and Covid are positive and significant, suggesting the analysts with an East Asian ethnicity have higher forecast error in their forecasts after the COVID-19 shock. In terms of economic magnitude, COVID-19 shock increases the forecast error by 11.9% to 19.0% for East Asian analysts, relative to the mean forecast error of analysts of other ethnicities. This result further confirms the main finding that migration fears deteriorate information acquisition and production for minority analysts and results in lower forecast accuracy and higher pessimism, especially for analysts from the ethnicity group who face more intensive xenophobia and discriminations.

Cross-sectional Analysis

To elucidate the underlying mechanisms of the observed association between heightened fear and anxiety and the inaccuracies and pessimism in financial analysts' forecasts, this section undertakes a detailed cross-sectional analysis. By conducting split sample tests, I aim to parse out how different factors may moderate or amplify the impact of macro-level racial sentiments on forecast accuracy and outlook. The variables considered for splitting the sample include the analysts' general experience, stock return volatility, surname favorability, and gender. These dimensions are selected based on the premise that they could represent meaningful variations in how macro-sentiment influences analyst forecasting behavior. In dissecting the interaction between elevated fear and anxiety and its impact on analyst forecast inaccuracy, I employ a methodological approach of splitting the sample based on quantiles which use the median values as thresholds.

I start my analysis by firstly examining how analysts' general experience play a role. The analysis reveals a notable difference in the impact of fear and anxiety between the two experience groups. The interaction term's coefficient is significantly larger and more significant for analysts with greater than median general experience compared to their less experienced counterparts. This suggests that analysts with a higher level of general experience are disproportionately affected by increases in fear and anxiety. Typically, more experienced analysts, who are often older, rely heavily on personal networks and direct connections with management to gather information. This reliance becomes a double-edged sword during periods of heightened fear and anxiety.

Consistent with the insights from Flam et al. (2023), the findings underscore the particular hurdles minority analysts face in accessing information. For analysts with extensive experience, the heightened fear and anxiety cause additional hurdles that increase the cost of information acquisition. This scenario suggests that during turbulent times, the advantages of experience and established networks are overshadowed by the challenges in accessing and processing critical information, especially for minority analysts who might already encounter barriers in their professional environments.

Following the methodological approach used to analyze the impact of analysts' general experience, I extend the investigation to examine how stock return volatility interacts with the effects of fear and anxiety on forecast accuracy. Given the established significance of information acquisition costs identified earlier, I conjecture that the implications of an elevated fear index would be more pronounced for firms that present a higher challenge in forecasting due to their inherent volatility.

To test this hypothesis, the sample is again divided into two quantiles based on the median level of stock return volatility, aiming to distinguish between firms that are relatively easier to forecast from those that are not. The analysis, detailed in the second column of Table 38, affirms the hypothesis. The results are significantly more pronounced for firms with higher inherent volatility, suggesting that the negative impact of macro-sentiment on forecast accuracy is magnified in contexts where the forecasting task is inherently more difficult. This finding further corroborates the notion that the information environment plays a pivotal role in shaping the forecasts of financial analysts, particularly under conditions of elevated fear and anxiety.

In further dissecting the layers that influence forecast accuracy and pessimism among financial analysts, I investigate the impact of surname favorability on analysts' forecasts, particularly under conditions of heightened fear and anxiety. Drawing on insights from Jung et al. (2019), which suggest that analysts with less favorable surnames encounter negative biases in how their analyses are received by the market, this segment explores the intersection of surname favorability with external socio-political stressors. The prevailing literature, including findings by Wright and Bower (1992), Wann, Dolan, McGeorge, and Allison (1994), Forgas (1995), and Kang (2020), highlights how mood and anxiety can detrimentally impact economic assessments. It stands to reason, therefore, that minority analysts bearing less favorable surnames might face exacerbated adverse effects during periods marked by significant fear and anxiety, given their forecasts are already prone to being overlooked or undervalued.

To investigate this hypothesis, the sample is segmented based on the favorability of analysts' surnames, and the analysis, presented in the third column of Table 38, reveals telling results. Consistent with the study's predictions, it is found that analysts with less favorable surnames endure more severe negative impacts on their forecast accuracy and pessimism during times of elevated fear and anxiety. This outcome not only underscores the compounded challenges faced by these analysts in an already biased market environment but also sheds light on the additional layers of complexity that macro-sentiment introduces to the task of financial forecasting.

Last but not least, exploring gender dynamics within the context of financial analysts' forecasts, especially against the backdrop of elevated fear and anxiety, introduces a critical layer of analysis. In the realm of financial analysis, female analysts constitute a notably smaller proportion of the profession, representing only about 10% of the analyst group. This minority status within the profession itself could potentially amplify the effects of public hostility on their information acquisition. Given the underrepresentation of females in the analyst population, this segment of the analysis seeks to discern whether female analysts experience a differentiated impact of fear and anxiety on their forecasts, relative to their male counterparts.

The last column of Table 38 is particularly illuminating in this regard. It presents the coefficient estimates that clearly demonstrate the interaction term between the Minority indicator and the Fear index is significantly more substantial and economically more impactful for female analysts. This finding indicates that the adverse effects of fear and anxiety on minority analysts' forecast accuracy are notably intensified by gender-based discrimination. Thus, female analysts, especially those from minority backgrounds, face a compounded challenge, where both their minority status and gender amplify the negative consequences of public hostility on their earnings per share forecasts.

This analysis underscores the importance of recognizing and addressing the unique challenges faced by female financial analysts in times of increased societal anxiety. It highlights the need for more inclusive policies and practices within the financial industry that account for the intersectional biases and barriers impacting analyst forecasts.

Conclusion

Acknowledging the well-documented impact of analysts' earnings forecasts on stock market reactions and the inherent informativeness of these forecasts, this study underscores that the accuracy and utility of such predictions are often compromised by exogenous external factors. These factors can impair the ability to accurately process and integrate firm-relevant information into forecasts, leading to potential inaccuracies and misinterpretations.

This paper explores the specific impact of macro-level racial sentiments on the accuracy and pessimism of earnings forecasts made by minority financial analysts. Through a comprehensive empirical framework that includes a difference-in-differences analysis centered around significant socio-political events, the study aims to illuminate how external socio-political dynamics, particularly those related to racial sentiment, influence the forecasting process.

The findings reveal a pronounced effect of heightened public fear and anxiety on the forecasting accuracy of minority analysts, differentiating their experiences from those of their non-minority counterparts. Specifically, during periods of increased racial tension, minority analysts' forecasts become significantly more inaccurate and pessimistic, highlighting the sensitivity of financial predictions to external racial sentiments. This sensitivity of financial predictions to external racial sentiments is further quantified, revealing that a one standard deviation surge in the Fear Index is linked to a 3% elevation in absolute forecast errors and a 10% amplification in forecast pessimism relative to the baseline average.

By shedding light on the intersections between financial analysis, minority representation, and external socio-political factors, this paper contributes significantly to the existing literature on sell-side financial analysts and Diversity, Equity, and Inclusion (DEI). It offers actionable insights for policymakers, brokerage houses, and investors, emphasizing the importance of considering the broader socio-political context in the interpretation and valuation of financial forecasts. The study advocates for strategies that mitigate the impact of external stressors on analysts' forecasting abilities, emphasizing the need for an informed and nuanced approach to financial forecasting that accounts for the complexities of a diverse and interconnected global market.

Conclusively, this research underscores the necessity for stakeholders to actively address and account for the identified challenges to enhance the accuracy and reliability of financial forecasts, especially in socio-politically turbulent times. It paves the way for future investigations into how socio-political influences intersect with financial market dynamics, aiming for a more equitable and informed financial analysis landscape.

Table 31 Summary statistics

This table provides the summary statistics of key variables on the 1990-2021 regression sample.

	N	Mean	Std. Dev.	p25	Median	p75
AFE	897708	.126	.285	.015	.04	.11
Forecast Bias	897708	.016	.154	-.032	0	.033
Minority	897708	.101	.301	0	0	0
Fear	897708	1.142	.481	.818	1.004	1.284
Size	897708	8.089	1.658	6.91	8.037	9.218
Tobin	897708	2.536	1.846	1.396	1.91	2.924
Female Analyst	897708	.1	.3	0	0	0
General Experience	897708	2.282	.764	1.792	2.398	2.833
Horizon	897708	3.707	.731	3.526	4.007	4.159
Brokage Size	897708	2.856	1.046	2.197	3.045	3.664
Number of Analysts	897708	2.019	.661	1.609	2.079	2.565
Number of Firms	897708	2.178	.792	1.792	2.398	2.708

Table 32 Baseline results – forecast accuracy

This table reports coefficient estimates of ordinary least squares regressions. I regress analyst absolute forecast error on the interaction of *Minority* and *Fear*. *AFE* is the absolute forecast error of the analyst, scaled by the closing stock price prior to the forecasting date. *Minority* is an indicator equal to one if the analyst's inferred ethnicity is *Black*, *Hispanic* or *Asian*. *Fear* is the migration fear index by Baker, Bloom, and Davis (2015,2016). Variable definitions for controls are provided in the appendix. Columns (1)-(3) feature firm-by-year fixed effects, while columns (4)-(6) have firm-by-year-by-quarter fixed effects. Analyst fixed effects are added in columns (2) and (5). Columns (3) and (6) introduce firm-by-analyst fixed effects. Standard errors are clustered at analyst and quarter level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES			AFE			
Minority X Fear	**0.005*** **	**0.005*** **	**0.005*** **	**0.005*** **	**0.005*** **	**0.005*** **
	(3.81)	**(3.07)**	**(2.86)**	**(4.24)**	**(3.65)**	**(3.68)**
Minority	-0.003**			-0.003**		
	(-2.00)			(-2.33)		
Fear	-0.004	-0.004	-0.004	-0.000	-0.000	0.000
	(-1.42)	(-1.43)	(-1.40)	(-0.13)	(-0.21)	(0.26)
Size	0.060***	0.060***	0.060***			
	(5.03)	(5.03)	(5.01)			
Tobin	-0.000	-0.000	-0.000			
	(-0.03)	(-0.00)	(-0.11)			
Female Analyst	0.001***			0.001		
	(2.91)			(1.17)		
General Experience	0.000	-0.000	-0.001	0.000	0.000	-0.001
	(0.24)	(-0.10)	(-0.77)	(0.00)	(0.37)	(-0.69)
Horizon	0.007***	0.006***	0.006***	0.009***	0.008***	0.008***
	(10.68)	(9.91)	(9.13)	(15.90)	(15.65)	(15.71)
Brokage Size	-0.001***	-0.001**	-0.001	-0.001***	-0.001*	-0.000
	(-3.47)	(-2.54)	(-1.58)	(-2.82)	(-1.84)	(-1.02)
Number of Analysts	0.001	0.000	0.000	0.004	0.004	0.003
	(0.31)	(0.05)	(0.01)	(0.59)	(0.74)	(0.55)
Number of Firms	-0.000	0.000	-0.000	-0.001	-0.000	-0.001***
	(-1.37)	(0.43)	(-0.59)	(-1.63)	(-0.67)	(-2.92)
Constant	-0.375***	-0.376***	-0.374***	0.090***	0.089***	0.094***
	(-3.75)	(-3.73)	(-3.65)	(6.61)	(6.95)	(7.23)
Observations	897,708	897,620	885,438	897,690	897,597	884,487
Adj. R-squared	0.712	0.713	0.702	0.872	0.874	0.877
Analyst FE	N	Y	N	N	Y	N
Firm X Analyst FE	N	N	Y	N	N	Y
Firm X Year FE	Y	Y	Y	N	N	N
Firm X Quarter FE	N	N	N	Y	Y	Y

Table 33 Baseline results – forecast bias

This table reports coefficient estimates of ordinary least squares regressions. I regress analyst signed forecast error on the interaction of *Minority* and *Fear*. *Forecast Bias* is the signed forecast error of the analyst, scaled by the closing stock price prior to the forecasting date. *Minority* is an indicator equal to one if the analyst's inferred ethnicity is *Black*, *Hispanic* or *Asian*. *Fear* is the migration fear index by Baker, Bloom, and Davis (2015,2016). Variable definitions for controls are provided in the appendix. Columns (1)-(3) feature firm-by-year fixed effects, while columns (4)-(6) have firm-by-year-by-quarter fixed effects. Analyst fixed effects are added in columns (2) and (5). Columns (3) and (6) introduce firm-by-analyst fixed effects. Standard errors are clustered at analyst and quarter level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Forecast Bias					
Minority X Fear	**-0.006****	**-0.007****	**-0.004****	**-0.007****	**-0.007*****	**-0.005****
	(-2.37)	**(-2.50)**	**(-2.39)**	**(-2.50)**	**(-2.71)**	**(-2.46)**
Minority	0.005	0.000	0.000	0.005	0.000	0.000
	(1.18)	(0.00)	(0.00)	(1.33)	(0.00)	(0.00)
Fear	-0.000	-0.000	-0.000	0.005	0.005	0.006
	(-0.50)	(-0.31)	(-0.58)	(0.77)	(0.76)	(0.92)
Size	-0.004***	-0.004***	-0.005***	0.000	0.000	0.000
	(-2.70)	(-2.62)	(-2.67)	(0.00)	(0.00)	(0.00)
Tobin	-0.000	-0.000	-0.000	0.000	0.000	0.000
	(-1.09)	(-1.05)	(-0.55)	(0.00)	(0.00)	(0.00)
Female	-0.005**	0.000	0.000	-0.005**	0.000	0.000
	(-1.99)	(0.00)	(0.00)	(-2.11)	(0.00)	(0.00)
General Experience	-0.003***	0.000	-0.001	-0.003***	-0.000	-0.001
	(-3.59)	(0.21)	(-0.27)	(-3.91)	(-0.01)	(-0.41)
Horizon	0.012***	0.012***	0.012***	0.017***	0.018***	0.017***
	(6.51)	(7.00)	(6.97)	(7.06)	(7.58)	(7.67)
Brokage Size	-0.002**	-0.001	-0.001	-0.002**	-0.001	-0.001
	(-2.28)	(-1.14)	(-0.85)	(-2.49)	(-1.29)	(-0.98)
Number of Analysts	0.004***	0.003***	0.002***	-0.004	-0.006	-0.009
	(5.54)	(4.46)	(2.95)	(-0.65)	(-0.87)	(-1.10)
Number of Firms	-0.002**	-0.003***	-0.003***	-0.003**	-0.003***	-0.004***
	(-2.29)	(-3.35)	(-3.21)	(-2.42)	(-3.51)	(-3.46)
Constant	0.002	-0.007	0.008	-0.040**	-0.049***	-0.040**
	(0.12)	(-0.44)	(0.41)	(-2.49)	(-3.06)	(-2.13)
Observations	897,515	897,427	885,248	897,497	897,404	884,301
Adj. R-squared	0.022	0.044	0.189	0.058	0.082	0.233
Analyst FE	N	Y	N	N	Y	N
Firm X Analyst FE	N	N	Y	N	N	Y
Firm X Year FE	Y	Y	Y	N	N	N
Firm X Year-Quarter FE	N	N	N	Y	Y	Y

Table 34 Analysis of 2016 presidential election

This table reports coefficient estimates of ordinary least squares regressions. I regress analyst absolute forecast error on the interaction of *Minority* and *Trump*. *AFE* is the absolute forecast error of the analyst, scaled by the closing stock price prior to the forecasting date. *Minority* is an indicator equal to one if the analyst's inferred ethnicity is *Black*, *Hispanic* or *Asian*. *Trump* is an indicator equal to one if the analyst forecast is issued after the 2016 Presidential Election. Variable definitions for controls are provided in the appendix. Columns (1)-(3) feature firm-by-year fixed effects, while columns (4)-(6) have firm-by-year-by-quarter fixed effects. Analyst fixed effects are added in columns (2) and (5). Columns (3) and (6) introduce firm-by-analyst fixed effects. Standard errors are clustered at analyst and quarter level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES			AFE			
Minority X Trump	**0.005*** **	**0.008*** **	**0.010*** **	**0.006*** **	**0.008*** **	**0.009*** **
	(3.38)	**(3.41)**	**(4.26)**	**(2.84)**	**(3.07)**	**(2.92)**
Minority	0.002			0.001		
	(1.60)			(1.41)		
Size	0.060***	0.060***	0.060***			
	(5.02)	(5.02)	(5.01)			
Tobin	-0.000	0.000	-0.000			
	(-0.00)	(0.03)	(-0.08)			
Female Analyst	0.001***			0.001		
	(2.92)			(1.19)		
General Experience	0.000	-0.000	-0.001	0.000	0.000	-0.001
	(0.26)	(-0.21)	(-0.86)	(0.01)	(0.24)	(-0.77)
Horizon	0.007***	0.006***	0.006***	0.009***	0.008***	0.008***
	(10.68)	(9.91)	(9.14)	(15.91)	(15.66)	(15.72)
Brokage Size	-0.001***	-0.001**	-0.001	-0.001***	-0.001*	-0.000
	(-3.48)	(-2.51)	(-1.56)	(-2.84)	(-1.83)	(-1.02)
Number of Analysts	0.001	0.000	0.000	0.004	0.004	0.003
	(0.33)	(0.07)	(0.03)	(0.60)	(0.74)	(0.56)
Number of Firms	-0.000	0.000	-0.000	-0.001	-0.000	-0.001***
	(-1.41)	(0.36)	(-0.63)	(-1.64)	(-0.71)	(-2.92)
Constant	-0.380***	-0.380***	-0.377***	0.090***	0.089***	0.095***
	(-3.78)	(-3.75)	(-3.67)	(6.90)	(7.44)	(7.68)
Observations	897,708	897,620	885,438	897,690	897,597	884,487
Adj. R-squared	0.712	0.713	0.702	0.872	0.874	0.877
Analyst FE	N	Y	N	N	Y	N
Firm X Analyst FE	N	N	Y	N	N	Y
Firm X Year FE	Y	Y	Y	N	N	N
Firm X Quarter FE	N	N	N	Y	Y	Y

Table 35 Parallel trends on 2016 presidential election

This table reports coefficient estimates of ordinary least squares regressions. I regress analyst absolute forecast error on the interaction of *Minority* and year indicators, which are centered around event years. *Minority* is an indicator equal to one if the analyst's inferred ethnicity is *Black*, *Hispanic* or *Asian*. *Event [-1]* represents the years preceding the event year by one year, while *Event [-2]* represents the years preceding the event year by two years. Similarly, *Event [+1]* represents the years following the event year by one year, and *Event [+2]* represents the years following the event year by two years. *Event [+3]* captures years that are three or more years after the event year. *Event [0]* represents the event years. *AFE* is the absolute forecast error of the analyst, scaled by the closing stock price prior to the forecasting date. Variable definitions for controls are provided in the appendix. Columns (1)-(3) feature firm-by-year fixed effects, while columns (4)-(6) have firm-by-year-by-quarter fixed effects. Analyst fixed effects are added in columns (2) and (5). Columns (3) and (6) introduce firm-by-analyst fixed effects. Standard errors are clustered at analyst and quarter level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES			AFE			
Minority X Event[-2]	0.001	0.003	0.004	0.001	0.003	0.003
	(0.39)	(1.23)	(1.46)	(0.38)	(1.18)	(1.18)
Minority X Event[-1]	0.001	0.003	0.004	0.001	0.003	0.003
	(0.21)	(0.97)	(1.08)	(0.25)	(0.98)	(1.04)
Minority X Event[0]	0.005*	0.006*	0.007*	0.005**	0.006**	0.006*
	(1.65)	(1.72)	(1.89)	(1.98)	(2.08)	(1.79)
Minority X Event[1]	0.011***	0.014***	0.015***	0.011***	0.014***	0.014***
	(3.05)	(3.17)	(3.87)	(3.12)	(3.40)	(3.81)
Minority X Event[2]	0.007*	0.010**	0.013***	0.007*	0.009**	0.011**
	(1.85)	(2.10)	(2.76)	(1.81)	(2.10)	(2.44)
Minority X Event[3]	0.003	0.008**	0.013***	0.004*	0.008***	0.011***
	(1.48)	(2.57)	(3.21)	(1.75)	(2.86)	(2.80)
Minority	0.001			0.001		
	(1.10)			(0.94)		
Constant	-0.380***	-0.380***	-0.377***	0.090***	0.089***	0.095***
	(-5.23)	(-5.22)	(-5.08)	(6.32)	(6.37)	(6.56)
Observations	897,708	897,620	885,438	897,690	897,597	884,487
Adj. R-squared	0.712	0.713	0.702	0.872	0.874	0.877
Controls	Y	Y	Y	Y	Y	Y
Analyst FE	N	Y	N	N	Y	N
Firm X Analyst FE	N	N	Y	N	N	Y
Firm X Year FE	Y	Y	Y	N	N	N
Firm X Quarter FE	N	N	N	Y	Y	Y

Table 36 Placebo tests of 2016 presidential election

This table reports coefficient estimates of ordinary least squares regressions. I regress analyst absolute forecast error on the interaction of a pseudo-minority indicator and *Trump*. *AFE* is the absolute forecast error of the analyst, scaled by the closing stock price prior to the forecasting date. *Minority* is an indicator equal to one if the analyst is randomly designated as a minority. *Trump* is an indicator equal to one if the analyst forecast is issued after the 2016 Presidential Election. Variable definitions for controls are provided in the appendix. Columns (1)-(3) are based on full sample. Columns (4)-(6) concentrate on the period surrounding the event year-quarter, with a span of eight quarters both preceding and succeeding it. Meanwhile, columns (7)-(9) are based on a sample, capturing a duration of twelve quarters on either side of the event year-quarter. All columns integrate firm-by-year-by-quarter fixed effects. Additionally, columns (2), (5), and (8) feature analyst fixed effects, while columns (3), (6), and (9) include firm-by-analyst fixed effects. Standard errors are clustered at analyst and quarter level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

	Full Sample			Event Window [-8,8]			Event Window [-12,12]		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Minority X Trump	**0.001**	**0.001**	**0.003**	**0.001**	**0.001**	**0.000**	**-0.000**	**0.000**	**0.000**
	(0.58)	**(0.55)**	**(1.18)**	**(0.27)**	**(0.48)**	**(0.03)**	**(-0.22)**	**(0.09)**	**(0.18)**
Minority	0.000	0.000	0.000	0.000	0.000	0.000	0.001	0.000	0.000
	(0.20)	(0.00)	(0.00)	(0.14)	(0.00)	(0.00)	(0.50)	(0.00)	(0.00)
Constant	0.090***	0.089***	0.095***	0.111***	0.105***	0.104***	0.114***	0.105***	0.103***
	(6.92)	(7.41)	(7.66)	(4.61)	(4.95)	(3.60)	(6.40)	(5.87)	(4.77)
Observations	897,690	897,597	884,487	180,810	180,773	177,826	267,525	267,491	263,671
Adj. R-squared	0.872	0.874	0.877	0.841	0.846	0.852	0.850	0.854	0.860
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Analyst FE	N	Y	N	N	Y	N	N	Y	N
Firm X Analyst FE	N	N	Y	N	N	Y	N	N	Y
Firm X Quarter FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Table 37 Analysis of 2019 Covid shock

This table reports coefficient estimates of ordinary least squares regressions. I regress analyst absolute forecast error on the interaction of *Covid* with six indicators. *AFE* is the absolute forecast error of the analyst, scaled by the closing stock price prior to the forecasting date. *Black*, *Indian*, *Hispanic*, *Muslim*, and *East Asian* are indicators equal to one if the analyst's inferred ethnicity is in the respective group. The ethnicity is determined by Ethnicolr algorithm. *Covid* is an indicator equal to one if the analyst forecast is issued after Donald Trump's "Chinese Virus" tweet. Variable definitions for controls are provided in the appendix. Columns (1)-(3) feature firm-by-year fixed effects, while columns (4)-(6) have firm-by-year-by-quarter fixed effects. Analyst fixed effects are added in columns (2) and (5). Columns (3) and (6) introduce firm-by-analyst fixed effects. Standard errors are clustered at analyst and quarter level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES			AFE			
Black X Covid	-0.011***	-0.007**	-0.009***	-0.007	-0.003	-0.005
	(-3.62)	(-2.39)	(-2.93)	(-1.03)	(-0.40)	(-0.65)
East Asian X Covid	**0.015*****	**0.017*****	**0.024*****	**0.016*****	**0.017*****	**0.024*****
	(4.39)	**(4.87)**	**(6.17)**	**(3.35)**	**(3.71)**	**(4.33)**
Indian X Covid	-0.001	-0.000	-0.001	0.001	0.001	-0.003
	(-0.50)	(-0.02)	(-0.49)	(0.25)	(0.36)	(-0.50)
Hispanic X Covid	-0.004	-0.008**	-0.007**	-0.002	-0.004	-0.004
	(-1.44)	(-2.24)	(-2.32)	(-0.55)	(-0.86)	(-0.57)
Muslim X Covid	-0.001	-0.002	0.005	0.001	-0.000	0.004
	(-0.33)	(-0.64)	(1.09)	(0.26)	(-0.02)	(0.66)
Muslim	-0.003			-0.003		
	(-1.54)			(-1.36)		
Black	0.000			0.001		
	(0.29)			(0.46)		
East Asian	0.000			-0.000		
	(0.14)			(-0.04)		
Indian	0.002**			0.002**		
	(2.49)			(2.21)		
Hispanic	-0.000			-0.000		
	(-0.43)			(-0.42)		
Constant	-0.376***	-0.377***	-0.374***	0.091***	0.091***	0.095***
	(-3.78)	(-3.75)	(-3.67)	(7.15)	(7.74)	(7.72)
Observations	887,618	887,530	875,439	887,216	887,123	874,100
Adj. R-squared	0.711	0.712	0.702	0.873	0.874	0.878
Controls	Y	Y	Y	Y	Y	Y
Analyst FE	N	Y	N	N	Y	N
Firm X Analyst FE	N	N	Y	N	N	Y
Firm X Year FE	Y	Y	Y	N	N	N
Firm X Quarter FE	N	N	N	Y	Y	Y

Table 38 Split sample analysis

This table presents split sample analysis based on analysts' general experience, stock return volatility, surname favorability, and gender. The results include coefficient estimates from ordinary least squares (OLS) regressions, adhering to the baseline specification outlined in equation (1). *AFE* is the absolute forecast error of the analyst, scaled by the closing stock price prior to the forecasting date. *Minority* is an indicator equal to one if the analyst's inferred ethnicity is *Black*, *Hispanic* or *Asian*. *Fear* is the migration fear index by Baker, Bloom, and Davis (2015,2016). Variable definitions for controls are provided in the appendix. Continuous variables are split at the median level. All columns include firm-by-year fixed effects. Standard errors are clustered at analyst and quarter level, with t-statistics reported in parentheses. *, **, *** indicate significance at the 10, 5, 1% levels, respectively. P-values for tests of coefficient equality (using the Wald Test) are presented in the final row.

	General Exp.		Stock Return Vol.		Surname Favoritism		Gender	
VARIABLES	>Median	<=Median	>Median	<=Median	>Median	<=Median	Male	Female
Minority X Fear	**-0.013*** **	-0.003	**-0.009** **	-0.003	-0.005	**-0.008** **	-0.005	**-0.015*** **
	(-2.80)	(-1.09)	**(-2.37)**	(-1.43)	(-1.13)	**(-2.33)**	(-1.65)	**(-3.44)**
Minority	0.010	0.002	0.009*	-0.000	0.007	0.002	0.004	0.007
	(1.20)	(0.56)	(1.75)	(-0.13)	(1.10)	(0.46)	(0.91)	(1.17)
Fear	0.001	-0.001	-0.001***	0.001	0.000	-0.001	-0.001	0.003
	(0.61)	(-0.77)	(-3.76)	(1.50)	(0.29)	(-0.77)	(-0.97)	(1.47)
Constant	-0.014	0.028	-0.002	0.051**	0.014	-0.003	-0.005	0.061
	(-0.52)	(1.07)	(-0.11)	(2.33)	(0.63)	(-0.15)	(-0.28)	(1.03)
Observations	416,054	473,287	447,699	449,814	473,458	415,690	806,959	85,749
Adj R-squared	0.722	0.735	0.701	0.747	0.721	0.733	0.679	0.698
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm X Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Wald - Test	.0041		.0336		.0088		.0466	

CHAPTER 5

CONCLUSION

This dissertation represents a significant advancement in our understanding of the intersections between diversity, political rhetoric, and financial behavior, contributing novel insights across three critical areas of finance and economics. Through a series of comprehensive essays, this work not only bridges gaps in the existing literature but also provides actionable recommendations for practitioners, policymakers, and scholars alike.

The first essay makes a pioneering contribution by offering large sample evidence on the implications of workplace diversity within financial advisory firms. By elucidating the mechanisms through which diversity enhances client representation and fiduciary duty fulfillment, this essay extends the literature on diversity and financial advisors, highlighting the profound implications for a range of stakeholders including practitioners, policymakers, and researchers.

In the realm of crowdfunding, the second essay presents groundbreaking findings on the impact of social and political climates on minority funding opportunities. It provides a nuanced understanding of how quarterly shifts in the social and political environment correlate with funding gaps for minority creators on platforms like Kickstarter. This contribution is particularly notable for demonstrating the material consequences of inflamed political rhetoric on minority funding beyond localized events or digital spaces, offering econometric evidence of the financial repercussions of migration fears and attitudes towards minorities.

The third essay enriches scholarly discussions on the challenges faced by sell-side financial analysts and the principles of Diversity, Equity, and Inclusion (DEI) within the financial sector. By examining how exogenous factors, particularly public sentiment and political discourse, exacerbate the difficulties minority analysts encounter in producing accurate forecasts, this essay offers critical insights relevant to a broad spectrum of stakeholders. The findings advocate for strategies to mitigate the impact of external stressors on analysts, thereby improving the reliability of financial forecasts in periods of heightened societal tension.

Collectively, these essays contribute significantly to our understanding of how diversity, political discourse, and societal attitudes intersect to influence financial markets and behaviors. They call attention to the need for inclusive practices and policies that recognize and address the unique challenges faced by minorities in financial contexts. As this body of work demonstrates, embracing diversity and fostering an equitable environment are not only ethical imperatives but also crucial for enhancing the efficiency and integrity of financial markets.

In shedding light on these complex dynamics, this dissertation not only fills important gaps in the existing literature but also sets the stage for future research to explore these interactions further. The implications of this research extend beyond academia, offering valuable insights for enhancing diversity and equity within the financial industry and contributing to the development of more inclusive and resilient financial markets.

APPENDIX A

VARIABLE DEFINITIONS FOR CHALLENGING THE STATUS QUO: RACIAL DIVERSITY AND FINANCIAL ADVISOR MISCONDUCT

Advisor Characteristics	
Misconduct Dummy	An indicator variable equal to one if the advisor commits a misconduct
Prior Misconduct	An indicator variable equal to one if the advisor has a prior misconduct in the last three years
Experience	The number of years the advisor is registered in the FINRA database
Series 63	An indicator variable equal to one if the advisor successfully completes the Series 63 exam, which includes state security regulations
Series 65&66	An indicator variable equal to one if the advisor successfully completes Series 63 or 65 exams, qualifying them to work as an investment advisor
Male	An indicator variable equal to one if the advisor is a male
White	An indicator variable equal to one if the advisor is white
Branch Characteristics	
Misconduct Dummy	An indicator variable equal to one if the branch experiences a misconduct
Misconduct	Total number of misconducts committed in the branch
Branch Diversity	Herfindahl–Hirschman Index as a proxy for branch diversity based on the ethnic composition of financial advisors within the branch
Entropy	Entropy measure of branch diversity based on the ethnic composition of financial advisors within the branch
Ethnicolr Diversity	HHI measure of branch diversity based on the ethnicities inferred by Ethnicolr name-classifier
Branch Size	Total number of advisors working in the branch
Branch Closure	An indicator variable set to one if the branch is terminated
Branch Turnover	The fraction of advisors leaving the branch
Firm Characteristics	
Firm Historical Misconduct	Cumulative percent of advisors in a firm with a past misconduct over a three-year window
Firm Size	Total number of advisors working in the firm
Firm Age	The number of years the firm is registered in the FINRA database

APPENDIX B

VARIABLE DEFINITIONS FOR MIGRATION FEAR AND MINORITY CROWD-FUNDING SUCCESS: EVIDENCE FROM KICKSTARTER

Crowding funding outcome	
Success	An indicator equal to one if the project is successfully funded.
Pledges/Goal	Total amount pledged to the project, scaled by the project goal. A cap is set at 125%.
Backers	Total number of backers who pledged any amount to the project.
Creator characteristics	
Minority	An indicator equal to one if the project creator is Black, Hispanic or Asian. It is inferred by using NamePrism algorithm.
Black, Hispanic, Asian	An indicator equal to one if the inferred probability is the largest among all ethnic groups.
Pr(Minority)	The probability inferred by NamePrism algorithm that the project creator is a minority summed over minority groups.
Census Minority	An indicator equal to one if the project creator is Black, Hispanic or Asian. It is inferred by using ethnicity classifier developed by Ambekar et al. (2009; known as Ethnicolr). Calibrated using the Census data, the method uses a deep learning method to classify names into ethnic groups. Specifically, the Census Bureau provides data on the racial distribution of last names. A LSTM (long short-term memory) model is trained to assign race to names in proportion to how names are distributed across racial groups. See https://github.com/appeler/ethnicolr.
Female	An indicator equal to one if the project creator is Female. It is inferred by matching the creator name with the gender data published on https://github.com/lmullen/gender by Lincoln Mullen.
Creator characteristics	
Fear	Migration Fear Index created by Baker, Bloom, and Davis (2015, 2016) divided by 100. The index represents the total number of newspaper articles including any of the predefined terms, scaled by the total number of newspaper articles in the same calendar quarter. Predefined terms: "immigration, migration, assimilation, migrant, immigrant, asylum, refugee, open borders, border control, Schengen, human trafficking" falls under the migration term set; "anxiety, panic, bomb, fear, crime, terror, worry, concern, violent" in the fear term set. See https://www.policyuncertainty.com/immigration_fear.html.
Google SVI	The decile score of Google Search Volume Index that is calculated by using Google Trends. The index is based on the query frequency of ten most related words of each chosen keyword of the Migration Fear Index. The index is the first component of principal component analysis of 110 keywords (Law and Zuo 2021; Da et al. 2014).

Project characteristics	
Goal	The target amount of funding determined by the project creator.
Horizon	The duration that the project will be kept posted for funding on Kickstarter.
Total projects	Total number of projects submitted by the same creator in the same year-quarter.
Length	The length of the project description.
Self mention	An indicator equal to one if the project creator self-mentioned himself/herself in the project description.
Staff picked	An indicator equal to one if the project is staff picked.

APPENDIX C

VARIABLE DEFINITIONS FOR MIGRATION FEAR AND FORECAST ACCURACY OF ETHNIC MINORITY ANALYSTS

Analyst Characteristics	
AFE	Analyst absolute forecast error, determined as the absolute difference between the analyst's EPS forecast and the actual EPS. The difference is scaled by the closing stock price prior to the forecasting date
Forecast Bias	Analyst forecast error, determined as the difference between the analyst's EPS forecast and the actual EPS. The difference is scaled by the closing stock price prior to the forecasting date
Minority	An indicator equal to one if the analyst is Black, Hispanic or Asian. It is inferred by using the NamePrism algorithm
Black, Hispanic, Asian	An indicator equal to one if the inferred probability is the largest among all ethnic groups based on the NamePrism algorithm
East Asian, Black, Hispanic, Indian, Muslim	An indicator equal to one if the inferred probability is the largest among all ethnic groups based on the Ethnicolr algorithm
Fear	The Migration Fear Index, developed by Baker, Bloom, and Davis (2015, 2016), is scaled down by a factor of 100. This index gauges the proportion of newspaper articles containing predefined terms related to migration and fear relative to the total articles published in the same calendar quarter. The migration related terms include "immigration, migration, assimilation, migrant, immigrant, asylum, refugee, open borders, border control, Schengen, and human trafficking", while the fear-associated terms consist of "anxiety, panic, bomb, fear, crime, terror, worry, concern, and violent"
General Experience	Logged number of years the analyst has been recorded in the IBES database
Horizon	Logged number of days between the firm's fiscal date and the analyst's forecast date
Female Analyst	An indicator equal to one if the analyst is a female
Brokage Size	Logged number of analysts at the brokerage house with which the analyst is affiliated in the forecasting year-quarter period
Number of Firms	Logged number of firms that the analyst is following in the same forecasting year-quarter period
Firm Characteristics	
Size	Logged total assets
Tobin's Q	Book value of assets plus market value of equity minus book value of equity, scaled by total assets
Number of Analysts	Logged number of analysts following the same company in each forecasting year-quarter period

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